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" Nec aranearum sane textus ideo melior quia ex se fila gignunt, nec noster vilior quia ex alienis libamus ut apes." Just. Lips. *Polit.* lib. i. cap. 1. Not.

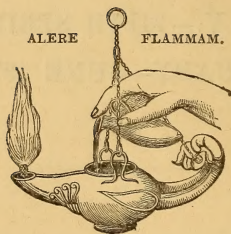## VOL. V.—FIFTH SERIES.

JANUARY—JUNE 1878.
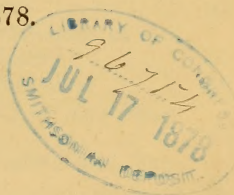
"Meditationis est perscrutari occulta; contemplationis est admirari perspicua . . . . . Admiratio generat quæstionem, quæstio investigationem, investigatio inventionem."—*Hugo de S. Victore.*

—"Cur spirent venti, cur terra dehiscat,
Cur mare turgescat, pelago cur tantus amaror,
Cur caput obscura Phœbus ferrugine condat,
Quid toties diros cogat flagrare cometas;
Quid pariat nubes, veniant cur fulmina cœlo,
Quo micet igne Iris, superos quis conciat orbes
Tam vario motu."

J. B. Pinelli ad Mazonium.

ALERE   FLAMMAM.

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ERRATUM IN VOL. IV.

Page 461, lines 16–21, *for* P_0 *read* P.

ERRATA IN VOL. V.

In footnote, page 206, *for* Plücker *read* Rücker.

Page 216, lines 3 and 4, *for*

$$7 \quad 4 \quad 5 \quad \vdots \quad \vdots \quad 6 \quad 2 \quad 5$$
$$a_3 \; a_2 \; a_1 \; \vdots \; a_0{}' \; \vdots \; a_{-1} \; a_{-2} \; a_{-3}$$

read $7 \quad 4 \quad 5 \quad \vdots \quad \cdot \quad \vdots \quad 6 \quad 2 \quad 5$
$$a_3 \; a_2 \; a_1 \; \vdots \; a_0 \; \vdots \; a_{-1} \; a_{-2} \; a_{-3}$$

I. *On a Form of Daniell Cell convenient as a Standard of Electromotive Force. By* Oliver J. Lodge, *D.Sc.*[*]

[Plate I.]

A LTHOUGH a volt is the formal unit of electromotive force, yet it happens in practice that differences of potential get stated as equal to so many Daniell cells more frequently than any thing else, showing that there is some decided convenience in this mode of statement, a convenience partly owing, no doubt, to the fact that a freshly set-up Daniell is a tolerably uniform and easily reproduced standard. An ordinary Daniell, however, is by no means suitable as a standard, because of the diffusion of the copper-liquid through the porous cell. This defect must obtain in any cell where two liquids separated by a porous partition are employed; and hence attempts have been made to construct standard cells with solid electrolytes, or with mercury instead of copper salts, as in the little cell devised by Mr. Latimer Clark, which, though not absolutely constant, is still, I suppose, the best for its special purpose. But all cells with solid electrolytes are extremely inconstant, in the sense that they suffer greatly from short-circuiting and take some time to recover themselves; and there are some other inconveniences attending the use of a large number of Clark's cells.

A convenient Daniell Cell with high internal Resistance.

Of all known cells, a Daniell charged with sulphate of zinc and sulphate of copper seems to be the most perfect—in this

respect, that the materials remain always the same during action except that the sulphate of zinc gradually increases in quantity, a difference which scarcely affects the electromotive force. Almost the only defect in the constancy of a cell so charged is due to the fact that the two liquids diffuse into each other, for which reason the battery cannot retain its original state after it has stood for some time. Any thing equivalent to a porous partition is quite useless for keeping the liquids separate; and the only plan seems to be to provide as long a column of liquid as possible for the copper salt to diffuse through.

This is done in a compact and simple manner in the cell represented in fig. 1 (Pl. I.). A wide-mouthed bottle (or a tall jar) is fitted with a cork through which passes a wide glass tube open at both ends. To the lower end of this tube a short closed tube (like a test-tube) is tied with silk thread; a long strip of sheet zinc is put down the open tube; and a copper wire, recurved at the bottom and coated with sealing-wax except at its two ends, is passed through the cork to the bottom of the closed tube, where it is imbedded in a few crystals of copper sulphate. The bottle is then nearly filled with dilute sulphate of zinc, and the cork with the tube is inserted, the latter being so arranged that the end of the zinc strip and the mouth of the short tube are both below the surface of the liquid. In a short time a strong solution of sulphate of copper forms at the bottom of the closed tube where the copper wire is bare, and it gradually diffuses upward; but in order to reach the zinc it has to diffuse itself all through the water of the bottle and then up the long tube containing the zinc; and this takes a long time, though it certainly does take place to some extent in a week or so.

But when I want to put the cell by for any length of time, I pull the long tube a little higher up through the cork, so that the mouth of the short tube emerges above the liquid and thus entirely prevents diffusion. The zinc strip is also raised out of the liquid by the same action. It is convenient to have the cork fitting pretty air-tight; or else evaporation may go on from the edges of the tube, and the salts which crystallize there may continue the diffusion slowly.

The copper wire need not be covered with wax or any thing; but if it were not, its upper parts would assist in the action until they were polarized; and hence the internal resistance would be liable to vary, which is not desirable. The internal resistance of such a cell is always rather high: for instance, in the one of which fig. 1 is a portrait, the bottle stands about 6 inches high, and the internal resistance is about 500 ohms

when arranged as shown ; but of course it depends greatly on the position of the tubes, and also somewhat on the temperature. Hence it is not to be regarded as giving necessarily a very constant current, but rather as a cell which can be used for a long time and yet keep its electromotive force nearly unchanged.

I have also made a set of small cells on the same principle, with ordinary quilled tubing for the tubes, and with test-tubes for the containing vessels, making the connexions by twisting the thin copper wire of one cell round the projecting tube (with the zinc bent down springily over it) of the next. A large number of such cells may be quickly made and arranged in ordinary test-tube stands ; and they are convenient for many purposes, such as capacity- or insulation-testing, where high electromotive force is required *. The whole rack of cells was once accidentally upset ; but though a little liquid escaped from the open ends of the zinc-tubes, the copper-liquid remained steady at the bottom of its tube without visible disturbance.

A Cell for a Standard of Electromotive Force.

Fig. 2 shows a bottle about 3 inches high, which I have made to act as a standard of electromotive force. It differs in no essential respect from fig. 1, except that the mouth of the tube containing the copper-solution never dips below the surface of the liquid, but always projects $\frac{1}{4}$ inch above it. The other or open tube does not project at all above the cork ; and its lower end is drawn out and coiled round so as still further to retard the passage of the copper-liquid to the zinc. The zinc, which should be pure, is supported at the right height by a pin thrust through it. The closed tube is proportionally longer than in fig. 1 ; it is nearly filled with pure sulphate-of-copper solution, a few crystals being placed at the bottom ; and it is tied to the other tube, as before, with silk thread (which appears not to rot). The copper wire is gutta-percha-covered with its ends bared. The bottle is filled nearly to its neck with very dilute sulphate of zinc ; and the cork is then inserted air-tight.

No mixing of the liquids is now possible ; but conduction still takes place over the damp surface of the glass tube, especially if, before use, the whole bottle be slightly inclined so as to wet the edges of the tube. The slight film of zinc-salt thus formed, being hygroscopic and being in a saturated atmo-

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* I suppose that by using platinum instead of copper wire, and strong nitric acid or else sulphuric acid and bichromate of potash instead of the copper salt, one could nearly double the electromotive force, though with some loss of constancy.

sphere, will keep the top of the tube sufficiently moist for an immense time.

The only possible changes which can go on in this cell are in the zinc and the solution in immediate contact with it. This solution can at any time be drawn off with a pipette and replaced by fresh, without greatly affecting the liquid in the bottle (if the cork be air-tight); and the zinc can still more easily be taken out and replaced by a new piece.

I have described the cell as at present made; but if there were any chance of its coming into use as a standard, a few modifications might be introduced. Thus the zinc might be a short rod with an india-rubber collar fitting the tube and with a short copper wire attached to it, which should project above the cork instead of the zinc, the joint being a little way down the tube and protected by a coat of varnish from damp air. A set of experiments would have to be made to determine the dependence of electromotive force on temperature; and then a thermometer with a short scale might be fixed in each cork.

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II. *On the Thermoelastic, Thermomagnetic, and Pyroelectric Properties of Matter.* By WILLIAM THOMSON, *M.A., late Fellow of St. Peter's College, Cambridge, Professor of Natural Philosophy in the University of Glasgow*[*].

1. A BODY which is either emitting heat, or altering its dimensions against resisting forces, is doing work upon matter external to it. The mechanical effect of this work in

[*] [This paper is in the main a reprint from an article which appeared under the title " On the Thermoelastic and Thermomagnetic Properties of Matter, Part I.," in April 1855, in the first number of the 'Quarterly Journal of Mathematics,' but which was confined to the thermoelastic part of the subject. The continuation, in which it was intended to make a similar application of thermodynamic principles to magnetic induction, was never published or written; but the results which it should have contained were sufficiently indicated in a short article on "Thermomagnetism," which I wrote at the request of my friend and colleague the late Professor J. P. Nichol for the second edition of his 'Cyclopædia,' published in 1860, and which I include in the present reprint. The addition of " Pyro-Electricity," which I now make to the title of the former article, is justified by another short quotation from the second edition of Nichol's 'Cyclopædia' (article " Thermo-Electricity, Division I.—Pyro-Electricity, or Thermo-Electricity of Nonconducting Crystals"), and a short addition, now written and published for the first time, in which the same thermodynamic principles are applied to this form of thermoelectric action.

Several additions both in the shape of text and footnote are appended in the course of the reprint. These are all distinguished by being enclosed in brackets, [ ].]

one case is the excitation of thermal motions, and in the other the overcoming of resistances. The body must itself be alter- ing in its circumstances, so as to contain a less store of energy within it, by an amount precisely equal to the aggregate value of the mechanical effects produced ; and conversely, the ag- gregate value of the mechanical effects produced must depend solely on the initial and final states of the body, and is there- fore the same, whatever be the intermediate states through which the body passes, provided the *initial* and *final* states be the same.

2. The *total intrinsic energy* of a body might be defined as the mechanical value of all the effect it would produce, in heat emitted and in resistances overcome, if it were cooled to the utmost, and allowed to contract indefinitely or to expand inde- finitely according as the forces between its particles are attrac- tive or repulsive, when the thermal motions within it are all stopped ; but in our present state of ignorance regarding per- fect cold, and the nature of molecular forces, we cannot deter- mine this " total intrinsic energy " for any portion of matter ; nor even can we be sure that it is not infinitely great for a finite portion of matter. Hence it is convenient to choose a certain state as standard for the body under consideration, and to use the unqualified term *intrinsic energy* with reference to this standard state ; so that the " intrinsic energy of a body in a given state " will denote the mechanical value of the effects the body would produce in passing from the state in which it is given, to the standard state—or, which is the same, the me- chanical value of the whole agency that would be required to bring the body from the standard state to the state in which it is given.

3. In Part V.* of a series of papers on the Dynamical Theory of Heat, communicated to the Royal Society of Edinburgh, a system of formulæ founded on propositions established in Part I.† of the same series of papers, and expressing, for a given fluid mass, relations between its pressure, its thermal capacities, its intrinsic energy (all considered as functions of its temperature and volume), and Carnot's function of the temperature, were brought forward for the purpose of pointing out the importance of making the *intrinsic energy* of a fluid in different states an object of research along with the other elements which have hitherto been considered, and partially investigated in some cases. In the present communication a similar mode of treatment, extended to include solid bodies, unmagnetic [and unelectrified], or magnetized [or electrified]

* Trans. Roy. Soc. Edinb. December 15, 1851.
† Ibid. March 17, 1851.

in any way, is shown to lead to the most general possible theory of elasticity, whether of solids or fluids, and to point out various thermodynamic properties of solids and various thermal effects of magnetism [and of electricity] not hitherto discovered.

Section I.—*Elasticity of Solids or Fluids not subjected to Magnetic Force.*

4. Let $x, y, z, \xi, \eta, \zeta$ be six independent variables expressing the mechanical condition of a homogeneous solid mass, homogeneously strained in any way*, and let t be its temperature ; and (in accordance with the preceding explanations) let e denote its intrinsic energy, reckoned from a certain "standard state" defined by particular values, $x_0, y_0, z_0, \xi_0, \eta_0, \zeta_0, t_0$, on which its physical condition depends. Thus, if ϕ denotes a certain function depending on the nature of the substance, and vanishing for the values $x_0, y_0, \ldots t_0$ of the independent variables, we have

$$e = \phi(x, y, z, \xi, \eta, \zeta, t) ; \quad . \quad . \quad . \quad (1)$$

and a knowledge of the function ϕ [with besides a knowledge of w for one particular temperature†] comprehends all the thermoelastic qualities of the solid.

5. Now let us suppose the body to be strained so as to pass from the mechanical state $(x_0, y_0, z_0, \xi_0, \eta_0, \zeta_0)$ to $(x, y, z, \xi, \eta, \zeta)$ while it is constantly kept at the temperature t ; and let H denote the quantity of heat that must be supplied to it during this process to prevent its temperature from being lowered (a quantity which of course is zero, or negative, for such strains as cause no thermal effects, or which cause positive evolutions of heat). Let the body be brought back to its mechanical condition $(x_0, y_0, z_0, \xi_0, \eta_0, \zeta_0)$ through the same or any other of all the infinitely varied successions of states by which it may be made to pass from one to the other of the two which have been named, its temperature being kept always at t. Then, by the second Fundamental Law of the Dynamical Theory of Heat (see Trans. Roy. Soc. Edinb. May 1, 1854, p. 126), we must have

$$\frac{H}{t} + \frac{H'}{t} = 0,$$

and therefore $H' = -H$.

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* The terms *a strain*, or *to strain*, are used simply with reference to alterations of dimensions or form in a solid—the forces by which "a strain" is produced being called the *straining tensions or pressures*, or sometimes merely *the tensions or pressures*, to which the solid is subjected. This distinction of terms is adopted in accordance with the expressions used by Mr. Rankine in his paper on the Elasticity of Solids (Cambridge and Dublin Mathematical Journal, February 1851).

† [See equations (10), (11) of § 7 below.]

6. We conclude that the quantity of heat absorbed by the
body in being strained from one state to another at the same
temperature is quite independent of the particular succession of
states through which it is made to pass, provided it has through-
out the same temperature.   Hence we must have

$$H = \psi(x, y, z, \xi, \eta, \zeta, t) - \psi(x_0, y_0, z_0, \xi_0, \eta_0, \zeta_0, t), \quad \cdot \quad (2)$$

where ψ denotes a function of the variables.   Now the me-
chanical value of the heat taken in by the body while it passes
from one condition to the other, together with the work spent
in compelling it to do so, constitutes the whole augmentation
of mechanical energy which it experiences ; so that if ϵ denote
this augmentation—that is, if

$$\epsilon = \phi(x, y, z, \xi, \eta, \zeta, t) - \phi(x_0, y_0, z_0, \xi_0, \eta_0, \zeta_0, t), \quad \cdot \quad (3)$$

and if w denote the work done by the applied forces and J the
mechanical equivalent of the thermal unit, we have

$$\epsilon = w + JH. \quad \cdot \quad \cdot \quad \cdot \quad \cdot \quad \cdot \quad \cdot \quad (4)$$

From this we conclude that the work required to strain the
body from one to another of two given mechanical states,
keeping it always at the same temperature, is independent of
the particular succession of mechanical states through which
it is made to pass, and is always the same when the initial and
final states are the same.   This theorem was, I believe, first
given by Green (as a consequence of the most general con-
ceivable hypothesis that could be framed to explain the mutual
actions of the different parts of a body on which its elasticity
depends), who inferred from it that there cannot be 36, but only
21, independent coefficients [or "moduluses"] of elasticity, with
reference to axes chosen arbitrarily in any solid whatever.   It
is now demonstrated as a particular consequence of the Second
General Thermodynamic Law.   It might at first sight be re-
garded as simply a consequence of the general principle of
mechanical effect ; but this would be a mistake, fallen into
from forgetting that heat is in general evolved or absorbed
when a solid is strained in any way ; and the only absurdity
to which a denial of the proposition could lead would be the
possibility of a self-acting machine going on continually draw-
ing heat from a body surrounded by others at a higher tem-
perature, without the assistance of any at a lower temperature,
and performing an equivalent of mechanical work.

7. The full expression of the Second Thermodynamic Law for
the circumstances of elastic force is, as is shown in the pas-
sage referred to above (Trans. Roy. Soc. Edinb. May 1, 1854,
p. 126), that if H_t, H'_t, &c. denote the quantities of heat emitted

from a body when at temperatures[*] t, t' respectively, during operations changing its physical state in any way, the sum $\Sigma \dfrac{\mathrm{H}_t}{t}$ must vanish for any cycle of changes, if each is of a perfectly reversible character, and if at the end of all the body is brought back to its primitive state in every respect. Let us consider, for instance, the following cycle, which obviously fulfils both conditions.

(I.) Let the body, initially in the state $(x_0, y_0, z_0, \xi_0, \eta_0, \zeta_0, t)$, be raised in temperature from t to t', its form and dimensions being maintained constant.

(II.) Let it be strained from the state $(x_0, y_0, z_0, \xi_0, \eta_0, \zeta_0)$ to the state $(x, y, z, \xi, \eta, \zeta)$, while its temperature is kept always at t'.

(III.) Let it be lowered in temperature from t' to t, its form and dimensions being retained.

(IV.) Let it be brought back to the mechanical state $(x_0, y_0, z_0, \xi_0, \eta_0, \zeta_0)$, while its temperature is kept constantly at t.

The quantities of heat taken in by the body in these successive operations are respectively :—

(I.) $\dfrac{1}{\mathrm{J}}\{\phi(x_0, y_0, z_0, \xi_0, \eta_0, \zeta_0, t') - \phi(x_0, y_0, z_0, \xi_0, \eta_0, \zeta_0, t)\}$,

because the difference of the whole mechanical energies is simply the mechanical value of the heat taken in or emitted in all cases in which no work is either done on the body or received by it in virtue of the action of applied forces ;

(II.) $\psi(x, y, z, \xi, \eta, \zeta, t') - \psi(x_0, y_0, z_0, \xi_0, \eta_0, \zeta_0, t')$,

according to the notation expressed by equation (2) above ;

(III.) $-\dfrac{1}{\mathrm{J}}\{\phi(x, y, z, \xi, \eta, \zeta, t') - \phi(x, y, z, \xi, \eta, \zeta, t)\}$,

and

(IV.) $-\{\psi(x, y, z, \xi, \eta, \zeta, t) - \psi(x_0, y_0, z_0, \xi_0, \eta_0, \zeta_0, t)\}$.

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[*] Reckoned on the absolute thermodynamic scale, according to which "temperature" is defined as the mechanical equivalent of the thermal unit divided by "Carnot's function." In a paper "On the Thermal Effects of Fluids in Motion," by Mr. Joule and myself, communicated to the Royal Society last June [1854], and since published in the Philosophical Transactions, it is shown that temperature on the absolute thermodynamic scale does not differ sensibly from temperature on the ordinary scale of the air-thermometer, except by the addition of a constant number, which we find to be about 273·7 for the Centigrade scale. Thus, on the system now adopted, the temperature of melting ice is 273·7, that of boiling water is 373·7, and differences of temperature are sensibly the same as on an ordinary standard Centigrade thermometer.

If we suppose $t'-t$ to be infinitely small, these expressions become respectively, in accordance with the previous notation:—

(I.)
$$\frac{1}{J}\frac{de_0}{dt}(t'-t),$$

where e_0 denotes the value of e for $(x_0, y_0, z_0, \xi_0, \eta, \zeta_0, t)$;

(II.)
$$H+\frac{dH}{dt}(t'-t);$$

(III.)
$$-\frac{1}{J}\frac{de}{dt}(t'-t);$$

(IV.)
$$-H.$$

Hence we have

$$\Sigma\frac{H_t}{t}=\frac{\frac{1}{J}\frac{de_0}{dt}(t'-t)}{\frac{1}{2}(t+t')}+\frac{H+\frac{dH}{dt}(t'-t)}{t'}$$
$$+\frac{-\frac{1}{J}\frac{de}{dt}(t'-t)}{\frac{1}{2}(t+t')}+\frac{-H}{t};$$

or, since $e-e_0$ is what we have denoted by ϵ,

$$\Sigma\frac{H_t}{t}=(t'-t)\left\{\frac{d\left(\frac{H}{t}\right)}{dt}-\frac{1}{J}\frac{d\epsilon}{tdt}\right\};$$

and the expression of the Second Thermodynamic Law becomes

$$\frac{d\left(\frac{H}{t}\right)}{dt}-\frac{1}{J}\frac{d\epsilon}{tdt}=0. \quad . \quad . \quad . \quad . \quad . \quad (5)$$

Eliminating ϵ from this by (4), we have

$$H=-\frac{t}{J}\frac{dw}{dt}; \quad . \quad . \quad . \quad . \quad . \quad . \quad (6)$$

and, eliminating H,

$$\epsilon=w-t\frac{dw}{dt}. \quad . \quad . \quad . \quad . \quad . \quad . \quad (7)$$

This is equivalent to

$$e=e_0+w-t\frac{dw}{dt}; \quad . \quad . \quad . \quad . \quad . \quad (8)$$

or, if N_0 denote the specific heat of the mass at any temperature t, when kept constantly in the mechanical state $(x_0, y_0, z_0, \xi_0, \eta_0, \zeta_0)$,

$$e=J\int_{t_0}^{t}N_0dt+w-t\frac{dw}{dt}, \quad . \quad . \quad . \quad (9)$$

an expression which shows how the " intrinsic energy " of the body may be determined from observations giving w as a function of the seven independent variables, and N_0 as a function of the temperature, for a particular set of values of the geometrical elements. Conversely, by (5) we have

$$H = \frac{t}{J} \int \frac{d\epsilon}{t\,dt} \cdot dt; \quad \ldots \ldots \quad (10)$$

and by (6) and (7), or simply by (4),

$$w = \epsilon - JH; \quad \ldots \ldots \ldots \quad (11)$$

which show how H and w may be determined for all temperatures from a knowledge of the intrinsic energy of the body, and of [one of] those functions themselves for a particular temperature.

8. Let K denote the specific heat of the body at any temperature t, when it is allowed or compelled to vary in form and dimensions with the temperature, according to any fixed law—that is, when each of the variables $x, y, z, \xi, \eta, \zeta$ is a given function of t; and let N denote what this becomes in the particular case of each of these elements being maintained at a constant value; or, which is the same, let N be the specific heat of the body at any temperature when maintained at constant dimensions $(x, y, z, \xi, \eta, \zeta)$. We have

$$JN = \frac{de}{dt}, \quad \ldots \ldots \ldots \ldots \ldots \quad (12)$$

$$JK = \frac{de}{dt} + \frac{d(JH)}{dx}\frac{dx}{dt} + \frac{d(JH)}{dy}\frac{dy}{dt} + \frac{d(JH)}{dz}\frac{dz}{dt}$$
$$+ \frac{d(JH)}{d\xi}\frac{d\xi}{dt} + \frac{d(JH)}{d\eta}\frac{d\eta}{dt} + \frac{d(JH)}{d\zeta}\frac{d\zeta}{dt}. \quad (13)$$

Since JH is equal to $e - w$, this expression may be modified as follows:—If D denote the total differential of e,

$$JK = \frac{De}{dt} - \left(\frac{dw}{dx}\frac{dx}{dt} + \frac{dw}{dy}\frac{dy}{dt} + \frac{dw}{dz}\frac{dz}{dt} \right.$$
$$\left. + \frac{dw}{d\xi}\frac{d\xi}{dt} + \frac{dw}{d\eta}\frac{d\eta}{dt} + \frac{dw}{d\zeta}\frac{d\zeta}{dt} \right). \quad \ldots \quad (13\,bis)$$

9. These equations may be applied to any kind of matter; and they express all the information that can be derived, from the general thermodynamic principles, regarding the relations between thermal and mechanical effects produced by condensations, rarefactions, or distortions of any kind. For the case of a fluid they become reduced at once to the forms investigated specially for fluids in my previous communications. Thus, if the mass considered be one pound of any kind of fluid, we may

take one of the six variables $x, y, z, \xi, \eta, \zeta$ as the volume v, which it is made to occupy in any particular condition, and the remaining five will not affect its physical properties, and will therefore disappear from all the preceding equations ; and the state of the fluid will be completely defined by the values of the two independent variables v, t. Then, if p denote the pressure, we must have

$$\frac{dw}{dv} = -p \, ;$$

since $-pdv$ is the work done upon a fluid in compressing it under pressure p from a volume v to a volume $v + dv$. But from (9) we have

$$\frac{de}{dv} = \frac{dw}{dv} - t \frac{d}{dt} \frac{dw}{dv},$$

and, therefore,

$$\frac{de}{dv} = t \frac{dp}{dt} - p, \quad \cdots \cdots \quad (14)$$

and

$$\frac{d(\mathrm{JH})}{dv} = t \frac{dp}{dt}. \quad \cdots \cdots \quad (14 \; bis)$$

Hence (13) becomes

$$\mathrm{JK} = \frac{de}{dt} + t \frac{dp}{dt} \left(\frac{dv}{dt} \right),$$

where $\left(\dfrac{dv}{dt} \right)$ expresses the assumed relation between the naturally independent variables v, t. If this be such that the pressure is constant, we have

$$\frac{dv}{dt} = \frac{\dfrac{dp}{dt}}{-\dfrac{dp}{dv}} \, ;$$

and, K being now the specific heat under constant pressure, we have finally

$$\mathrm{JK} = \frac{de}{dt} + \frac{t \left(\dfrac{dp}{dt} \right)^2}{-\dfrac{dp}{dv}}. \quad \cdots \cdots \quad (15)$$

10. These equations (14) and (15), together with the unmodified equation (12), which retains the same form in all cases, express the general thermodynamic relations between the intrinsic energy, the pressure, and the specific heats of a fluid. If we eliminate e, we have

$$\mathrm{JK} - \mathrm{JN} = \frac{t\left(\frac{dp}{dt}\right)^2}{-\frac{dp}{dv}} \quad \ldots \quad (16)$$

and
$$\frac{d(\mathrm{JN})}{dv} = t\frac{d^2p}{dt^2}, \quad \ldots \ldots (17)$$

which are the equations used to express those relations in a recent paper by Mr. Joule and myself, " On the Thermal Effects of Fluids in Motion " *.

If, instead of $\frac{\mathbf{J}dt}{t}$, we substitute μdt, considering μ as a function of Carnot's function of the temperature, they become identical with the two fundamental equations (14) and (16) given in Part III. of my first communication " On the Dynamical Theory of Heat " †.

11. To apply the preceding equations to a body possessing rigidity, it is necessary to take the form as well as the bulk into account, and therefore to retain, besides the temperature, six independent variables to express those elements.   There is, of course, an infinite variety of ways in which the form and bulk of a homogeneously strained body may be expressed by means of six independent variables.   Thus the lengths (three variables) and the mutual inclinations (three variables) of the edges of a parallelepiped enclosing always the same portion of the solid in all states of strain (which of course always remains a parallelepiped, provided the strain is homogeneous throughout the solid), may be chosen for the independent variables ; or we may choose the six elements of an ellipsoid enclosing always the same portion of the solid (which will always remain an ellipsoid however the solid be strained, provided it is strained homogeneously. Thus, let us actually take for x, y, z the lengths of three conterminous edges OX, OY, OZ of a certain parallelepiped of the solid, and for ξ, η, ζ the angles between the planes meeting in these edges respectively, the parallelepiped being so chosen that it becomes strained into a cube of unit dimensions, when the solid is in the particular state at which we wish to investigate its thermoelastic properties.

12. If then we take

$$x_0 = 1, \quad y_0 = 1, \quad z_0 = 1,$$
$$\xi_0 = \tfrac{1}{2}\pi, \quad \eta_0 = \tfrac{1}{2}\pi, \quad \xi_0 = \tfrac{1}{2}\pi,$$

and if we suppose $x, y, z, \xi, \eta, \zeta$ to differ infinitely little from $x_0, y_0, z_0, \xi_0, \eta_0, \zeta_0$ respectively, the actual state $(x, y, z, \xi, \eta, \zeta)$ will be one in which the body is strained from the state

* Transactions of the Royal Society, June 15, 1854.
† Transactions of the Royal Society of Edinburgh, March 17, 1851.

$(x_0, y_0, z_0, \xi_0, \eta_0, \zeta_0)$ by the edges of the cube being elongated by $x-x_0, y-y_0, z-z_0,$ and the angles meeting in three conterminous edges receiving augmentations of $\xi-\xi_0, \eta-\eta_0, \zeta-\zeta_0.$ It is clear, since the altered angles differ each infinitely little from a right angle, that the strains represented by $\xi-\xi_0,$ $\eta-\eta_0, \zeta-\zeta_0$ involve no change of volume, and are simple deformations, each of a perfectly definite kind, in the planes YOZ, ZOX, XOY respectively, and that the change of volume due to the six coexistent strains is actually an infinitely small augmentation amounting to

$$x-x_0+y-y_0+z-z_0.$$

13. Considering still $x-x_0$ &c. as each very small, we have the following development by Maclaurin's theorem, the zero suffixes to the differential coefficients being used for brevity to denote the values of the different coefficients at $(x_0, y_0, z_0, \xi_0, \eta_0, \zeta_0)$.

$$
\begin{aligned}
w={}&\left(\frac{dw}{dx}\right)_0(x-x_0)+\left(\frac{dw}{dy}\right)_0(y-y_0)+\left(\frac{dw}{dz}\right)_0(z-z_0)\\
&+\left(\frac{dw}{d\xi}\right)_0(\xi-\xi_0)+\left(\frac{dw}{d\eta}\right)_0(\eta-\eta_0)+\left(\frac{dw}{\zeta}\right)_0(\zeta-\zeta_0)\\
&+\tfrac{1}{2}\left\{\left(\frac{d^2w}{dx^2}\right)_0(x-x_0)^2+\left(\frac{d^2w}{dy^2}\right)_0(y-y_0)^2+\left(\frac{d^2w}{dz^2}\right)_0(z-z_0)^2\right.\\
&\quad+\left(\frac{d^2w}{d\xi^2}\right)_0(\xi-\xi_0)^2+\left(\frac{d^2w}{d\eta^2}\right)_0(\eta-\eta_0)^2+\left(\frac{d^2w}{d\zeta^2}\right)_0(\zeta-\zeta_0)^2\\
&+2\left(\frac{d^2w}{dy\,dz}\right)_0(y-y_0)(z-z_0)+2\left(\frac{d^2w}{dz\,dx}\right)_0(z-z_0)(x-x_0)\\
&\qquad\qquad+2\left(\frac{d^2w}{dx\,dy}\right)_0(x-x_0)(y-y_0)\\
&+2\left(\frac{d^2w}{d\eta\,d\zeta}\right)_0(\eta-\eta_0)(\zeta-\zeta_0)+2\left(\frac{d^2w}{d\zeta\,d\xi}\right)_0(\zeta-\zeta_0)(\xi-\xi_0)\\
&\qquad\qquad+2\left(\frac{d^2w}{d\xi\,d\eta}\right)_0(\xi-\xi_0)(\eta-\eta_0)\\
&+2\left(\frac{d^2w}{dx\,d\xi}\right)_0(x-x_0)(\xi-\xi_0)+2\left(\frac{d^2w}{dy\,d\eta}\right)_0(y-y_0)(\eta-\eta_0)\\
&\qquad\qquad+2\left(\frac{d^2w}{dz\,d\zeta}\right)_0(z-z_0)(\zeta-\zeta_0)\\
&+2\left(\frac{d^2w}{dx\,d\eta}\right)_0(x-x_0)(\eta-\eta_0)+2\left(\frac{d^2w}{dx\,d\zeta}\right)_0(x-x_0)(\zeta-\zeta_0)\\
&+2\left(\frac{d^2w}{dy\,d\zeta}\right)_0(y-y_0)(\zeta-\zeta_0)+2\left(\frac{d^2w}{dy\,d\xi}\right)_0(y-y_0)(\xi-\xi_0)\\
&+2\left(\frac{d^2w}{dz\,d\xi}\right)_0(z-z_0)(\xi-\xi_0)+2\left(\frac{d^2w}{dz\,d\eta}\right)_0(z-z_0)(\eta-\eta_0)+\&c.
\end{aligned}
\tag{18}
$$

14. According to the system of variables * which we have adopted, as set forth in § 12, when $x - x_0$ &c. are each infinitely small, x increasing corresponds to a motion of all the particles in a plane at a distance unity from YOZ, in directions perpendicular to this plane, through a space numerically equal to the increment of x; ξ increasing corresponds to a motion of all the particles at a distance unity from XOY, in directions parallel to YO, through a space equal to the increment of ξ, or to a motion of all the particles at a distance unity from XOZ, in directions parallel to ZO, through a space equal to the increment of ξ, or to two such motions superimposed, through any spaces respectively, amounting together to a quantity equal to the increment of ξ. Similar statements apply to the effects of variations of the other four variables. Hence, if P, Q, R denote the normal components of the superficial tensions experienced respectively by the three pairs of opposite faces of the unit cube of the solid in the state of strain in which we are considering it, and if S, T, U be the components, along the planes of the faces, of the actual tensions, taken in order of symmetry, so that S denotes the component, perpendicular to the edge opposite to OX, of the superficial tension in either of the faces meeting in that edge (which are equal for these two faces, or else the cube would not be in equilibrium, but would experience the effect of a couple in a plane perpendicular to OX), and T and U denote components, perpendicular respectively to OY and OZ, of the superficial tensions of the pairs of faces meeting in those edges, the work done on the parallelepiped during an infinitely small strain in which the variables become augmented by dx, dy, &c. respectively will be

$$P dx + Q dy + R dz + S d\xi + T d\eta + U d\zeta.$$

Hence, if the portion of matter of which the intrinsic energy is denoted by e, and to which the notation ϵ, w, &c. applies, be the matter within the parallelepiped referred to, we have

$$\left.\begin{array}{lll} \dfrac{dw}{dx} = \mathrm{P}, & \dfrac{dw}{dy} = \mathrm{Q}, & \dfrac{dw}{dz} = \mathrm{R}, \\[2mm] \dfrac{dw}{d\xi} = \mathrm{S}, & \dfrac{dw}{d\eta} = \mathrm{T}, & \dfrac{dw}{d\zeta} = \mathrm{U}. \end{array}\right\} \quad . \quad . \quad . \quad (19)$$

15. Using the development of w expressed by (18), we derive from these equations the following expressions for the six component tensions :—

---

* [A method of generalized stress and strain components is fully developed in "Elements of a Mathematical Theory of Elasticity," first published in the Transactions of the Royal Society for April 1856, and embodied in an article on "Elasticity," about to be published in the *Encyclopædia Britannica.*]

$$P = \left(\frac{dw}{dx}\right)_0$$
$$+ \left(\frac{d^2w}{dx^2}\right)_0 (x-x_0) + \left(\frac{d^2w}{dx\,dy}\right)_0 (y-y_0) + \left(\frac{d^2w}{dx\,dz}\right)_0 (z-z_0)$$
$$+ \left(\frac{d^2w}{dx\,d\xi}\right)_0 (\xi-\xi_0) + \left(\frac{d^2w}{dx\,d\eta}\right)_0 (\eta-\eta_0) + \left(\frac{d^2w}{dx\,d\zeta}\right)_0 (\zeta-\zeta_0);$$

$$Q = \left(\frac{dw}{dy}\right)_0$$
$$+ \left(\frac{d^2w}{dx\,dy}\right)_0 (x-x_0) + \left(\frac{d^2w}{dy^2}\right)_0 (y-y_0) + \left(\frac{d^2w}{dy\,dz}\right)_0 (z-z_0)$$
$$+ \left(\frac{d^2w}{dy\,d\xi}\right)_0 (\xi-\xi_0) + \left(\frac{d^2w}{dy\,d\eta}\right)_0 (\eta-\eta_0) + \left(\frac{d^2w}{dy\,d\zeta}\right)_0 (\zeta-\zeta_0);$$

$$R = \left(\frac{dw}{dz}\right)_0$$
$$+ \left(\frac{d^2w}{dz\,dx}\right)_0 (x-x_0) + \left(\frac{d^2w}{dz\,dy}\right)_0 (y-y_0) + \left(\frac{d^2w}{dz^2}\right)_0 (z-z_0)$$
$$+ \left(\frac{d^2w}{dz\,d\xi}\right)_0 (\xi-\xi_0) + \left(\frac{d^2w}{dz\,d\eta}\right)_0 (\eta-\eta_0) + \left(\frac{d^2w}{dz\,d\zeta}\right)_0 (\zeta-\zeta_0);$$

$$(20)$$

$$S = \left(\frac{dw}{d\xi}\right)_0$$
$$+ \left(\frac{d^2w}{d\xi\,dx}\right)_0 (x-x_0) + \left(\frac{d^2w}{d\xi\,dy}\right)_0 (y-y_0) + \left(\frac{d^2w}{d\xi\,dz}\right)_0 (z-z_0)$$
$$+ \left(\frac{d^2w}{d\xi^2}\right)_0 (\xi-\xi_0) + \left(\frac{d^2w}{d\xi\,d\eta}\right)_0 (\eta-\eta_0) + \left(\frac{d^2w}{d\xi\,d\zeta}\right)_0 (\zeta-\zeta_0);$$

$$T = \left(\frac{dw}{d\eta}\right)_0$$
$$+ \left(\frac{d^2w}{d\eta\,dx}\right)_0 (x-x_0) + \left(\frac{d^2w}{d\eta\,dy}\right)_0 (y-y_0) + \left(\frac{d^2w}{d\eta\,dz}\right)_0 (z-z_0)$$
$$+ \left(\frac{d^2w}{d\xi\,d\eta}\right)_0 (\xi-\xi_0) + \left(\frac{d^2w}{d\eta^2}\right)_0 (\eta-\eta_0) + \left(\frac{d^2w}{d\zeta\,d\eta}\right)_0 (\zeta-\zeta_0);$$

$$U = \left(\frac{dw}{d\zeta}\right)_0$$
$$+ \left(\frac{d^2w}{d\zeta\,dx}\right)_0 (x-x_0) + \left(\frac{d^2w}{d\zeta\,dy}\right)_0 (y-y_0) + \left(\frac{d^2w}{d\zeta\,dz}\right)_0 (z-z_0)$$
$$+ \left(\frac{d^2w}{d\zeta\,d\xi}\right)_0 (\xi-\xi_0) + \left(\frac{d^2w}{d\zeta\,d\eta}\right)_0 (\eta-\eta_0) + \left(\frac{d^2w}{d\zeta^2}\right)_0 (\zeta-\zeta_0).$$

$$(21)$$

16. These equations express in the most general possible man-

ner the conditions of equilibrium of a solid in any state of strain whatever at a constant temperature.   They show how the straining forces are altered with any infinitely small alteration of the strain.   If we denote by P_0 &c. the values of P &c. for the state $(x_0, y_0, z_0, \xi_0, \eta_0, \zeta_0, t)$, the values of $P-P_0$, $Q-Q_0$, $R-R_0$, $S-S_0$, $T-T_0$, $U-U_0$ given by these equations as linear functions of the strains $(x-x_0), (y-y_0), (z-z_0), (\xi-\xi_0), (\eta-\eta_0), (\zeta-\zeta_0)$, with twenty-one coefficients, express the whole tensions required to apply these strains to the cube, if the condition of the solid when the parallelepiped is exactly cubical is a condition of no strain, and in this case become (if single letters are substituted for the coefficients $\left(\dfrac{d^2w}{dx^2}\right)_0$ &c.) identical with the equations of equilibrium of an elastic solid subjected to infinitely small strains, which have been given by Green, Cauchy, Haughton, and other writers.   Many mathematicians and experimenters have endeavoured to show that in actual solids there are certain essential relations between these twenty-one coefficients [or moduluses] of elasticity.   Whether or not it may be true that such relations do hold for natural crystals, it is quite certain that an arrangement of actual pieces of matter may be made, constituting a homogeneous whole when considered on a large scale (being, in fact, as homogeneous as writers adopting the atomic theory in any form consider a natural crystal to be), which shall have an arbitrarily prescribed value for each one of these twenty-one coefficients.   No one can legitimately deny for all natural crystals, known and unknown, any property of elasticity, or any other mechanical or physical property, which a solid composed of natural bodies artificially put together may have in reality.   To do so is to assume that the infinitely inconceivable structure of the particles of a crystal is essentially restricted by arbitrary conditions imposed by mathematicians for the sake of shortening the equations by which their properties are expressed.   It is true experiment might, and does, show particular values for the coefficients for particular bodies ; but I believe even the collation of recorded experimental investigations is enough to show bodies violating every relation that has been imposed; and I have not a doubt that an experiment on a natural crystal, magnetized if necessary, might be made to show each supposed relation violated.   Thus it has been shown, first I believe by Mr. Stokes, that the relation which the earlier writers supposed to exist between rigidity and resistance to compression is not verified, because experiments on the torsion of wires of various metals, rods of india-rubber, &c. indicate, on the whole, less rigidity than would be expected, according to that relation, from their resistance to

compression, and less in different proportions for different metals. It is quite certain that india-rubber, jelly of any kind (ever so stiff), and gutta-percha are all of them enormously less rigid in proportion to their resistance to compression than glass or the metals; and they are all certainly substances which may be prepared so as to be at least as homogeneous as rods, wires, bars, or tubes of metals. From some experiments communicated to me by Mr. Clerk Maxwell, which he has made on iron wire by flexure and torsion, it appears highly probable that iron is more rigid in proportion to its resistance to compression than M. Wertheim's experiments on brass and glass show these bodies to be.

[17. Since the publication of this paper, the same conclusion as to the relative qualities of iron and brass has been arrived at by Everett (Transactions of the Royal Society, 1865 and 1866) as a result of fresh experiments made by himself on these substances—but an opposite conclusion with reference to two specimens of flint glass upon which he experimented, and which both showed greater rigidity in proportion to compressibility than either his own experiments or those of others had shown for iron or any other substance accurately experimented on. Far beyond these specimens of glass, with respect to greatness of rigidity in proportion to compressibility, is cork; which though not hitherto accurately experimented on, and though no doubt very variable in its elastic quality, shows obviously a very remarkable property, on which its use for corking bottles depends, viz. that a column of it compressed endwise does not swell out sidewise to any sensible degree, if at all. It is easy to construct a model elastic solid, on the plan suggested above, which shall actually show lateral shrinking when compressed longitudinally, and lateral swelling when pulled out longitudinally. The false theory, referred to above as having been first proved to be at fault by Stokes, gives for every kind of solid $\frac{1}{4}$ as the ratio of the lateral shrinking to the longitudinal elongation when a rod is pulled out lengthwise. The following Table (p. 18) shows how different are the values of that ratio determined by experiment on several real solids.]

18. The known fact that [many] gelatinous bodies, and the nearly certain fact that most bodies of all kinds, when their temperatures are raised, become less rigid to a much more marked extent than that of any effect on their compressibilities, are enough to show that neither the relation first supposed to exist, nor any other constant relation between compressibility and rigidity, can hold even for one body at different temperatures.

Substances.	Authority.	Ratio of lateral shrinking or swelling to longitudinal extension or shortening under the influence of push or pull on ends of a column of the substance.
Cork	General experience and some accurate measurements of diameter of a cork under various degrees of end-pressure, producing shortenings from small amounts up to as much as $\frac{7}{8}$ of the original length	0
Specimens of "crystal" glass.....................	} Wertheim	·33
A specimen of flint glass.	Everett (1865)	·26
Another specimen of flint glass	} Everett (1866)	·23
A specimen of brass	Wertheim	·34
Drawn brass rod	Everett (1866)	·47
Copper	W. Thomson	from ·40 to ·23
Iron	Clerk Maxwell	·27
Steel	Kirchhoff	·29
Cast steel	Everett (1866)	·31
Vulcanized india-rubber.	Joule	{ Less than ·5 by an exceedingly small amount.

19. Again, some of the relations which have been supposed to exist lead to three principal axes of elasticity. Many natural crystals do certainly exhibit perfect symmetry of form with reference to three rectangular axes, and therefore probably possess all their physical properties symmetrically with reference to those axes ; but as certainly many, and among them some of the best-known, of natural crystals do not exhibit symmetry of form with reference to rectangular axes, and possess the mechanical property of resisting fracture differently in different directions, without symmetry about any three rectangular axes—for instance, Iceland spar, which has three planes of greatest brittleness (" cleavage-planes "), inclined at equal angles to one another and to a common axis (the " optic axis " of the crystal). If, as probably must be the case, the elastic properties within the limits of elasticity have correspondence with the mechanical properties on which the brittleness in different directions depends, the last-mentioned class of crystals cannot have three principal axes of elasticity at right angles to one another. It will be an interesting inquiry to examine thoroughly the various directional properties of an elastic solid represented by the different coefficients (of which the entire number may of course be reduced from twenty-one to eighteen by a choice of axes), or by various combinations of them.

20. The general thermodynamic principles expressed above in the equations (6), (8), (12), and (13) enable us to determine the relations between the evolution of heat or cold by strains of any kind effected on an elastic solid, the variation of its elastic forces with temperature, and the differences and variations of its specific heats. Thus (6) gives at once, when the development of w expressed by (18) is used, and for $\left(\dfrac{dw}{dx}\right)_0$ &c. are substituted P &c., which are infinitely nearly equal to them, the following expression for the heat absorbed by an infinitely small straining, namely from $(x_0, y_0, z_0, \xi_0, \eta_0, \zeta_0)$ to $(x, y, z, \xi, \eta, \zeta)$:—

$$H = \frac{t}{J} \left\{ \frac{-dP}{dt}(x-x_0) + \frac{-dQ}{dt}(y-y_0) + \frac{-dR}{dt}(z-z_0) \right.$$
$$\left. - \frac{dS}{dt}(\xi-\xi_0) - \frac{dT}{dt}(\eta-\eta_0) - \frac{dU}{dt}(\zeta-\zeta_0) \right\}. \quad . \quad (22)$$

21. We conclude that cold is produced whenever a solid is strained by opposing, and heat when it is strained by yielding to, any elastic force of its own, the strength of which would diminish if the temperature were raised—but that, on the contrary, heat is produced when a solid is strained against, and cold when it is strained by yielding to, any elastic force of its own, the strength of which would increase if the temperature were raised. When the stress is a pressure, uniform in all directions, fluids may be included in the statement. Thus we may conclude as certain:—

(1) That a cubical compression of any elastic fluid or solid in an ordinary condition would cause an evolution of heat; but that, on the contrary, a cubical compression would produce cold in any substance, solid or fluid, in such an abnormal state that it would contract if heated, while kept under constant pressure.

(2) That if a wire already twisted be suddenly twisted further, always, however, within its limits of elasticity, cold will be produced; and that if it be allowed suddenly to untwist, heat will be evolved from itself (besides heat generated externally by any work allowed to be wasted, which it does in untwisting). For I suppose it is certain that the torsive rigidity of every wire is diminished by an elevation of temperature.

(3) That a spiral spring suddenly drawn out will become lower in temperature, and will rise in temperature when suddenly allowed to draw in. [This result has since been experimentally verified by Joule ("Thermodynamic Properties of Solids," Trans. Roy. Soc. 1858), and the amount of the effect

found to agree with that calculated, according to the preceding thermodynamic theory, from the amount of the weakening of the spring which he found by experiment.]

(4) That a bar or rod or wire of any substance with or without a weight hung on it, or experiencing any degree of end thrust, to begin with, becomes cooled if suddenly elongated by end pull or by diminution of end thrust, and warmed if suddenly shortened by end thrust or by diminution of end pull: except abnormal cases, in which, with constant end pull or end thrust, elevation of temperature produces shortening ; in every such case pull or diminished thrust produces elevation of temperature, thrust or diminished pull lowering of temperature.

(4′) That an india-rubber band suddenly drawn out (within its limits of elasticity) produces cold, and that, on the contrary, when allowed to contract, heat will be evolved from it. For it is certain that an india-rubber band with a weight suspended by it will expand in length if the temperature be raised. [Alas for overconfident assertion ! This is not true— at all events not true *in general* for either natural or vulcanized india-rubber, but only true for india-rubber in somewhat exceptional circumstances. It was founded on the supposition that india-rubber becomes less rigid when raised in temperature, which, besides, seeming to be expectable for solids generally, seemed to be experimentally proved for india-rubber by the familiar stiffness of common india-rubber in very cold weather. My original supposition is in fact correct for india-rubber which has become rigid by being kept at rest at a low temperature for some time. In this condition india-rubber was found by Joule to be cooled when suddenly stretched, and heated when the stretching weight was removed; and therefore, when in this condition, it is certain, from the thermodynamic principle, that a band of the substance bearing a weight will expand in length if the temperature is raised, and shrink when the temperature is lowered. But the very piece of india-rubber in which Joule found a cooling effect by pull when its temperature was 5° C., gave him a heating effect by pull, and a cooling effect on withdrawal of pull, when the temperature was 15° C. Joule experimented also on vulcanized india-rubber, and with it always found a heating effect when the substance was pulled out, and a cooling effect when it was allowed to shrink back. I pointed out to him that therefore, by thermodynamic theory, a vulcanized india-rubber band, when stretched by a constant weight of sufficient amount hung on it, must, when heated, pull up the weight, and when cooled, allow the weight to descend. This is an experiment which any one can make with the greatest ease by hanging a

few pounds weight on a common india-rubber band, and taking a red-hot coal, in a pair of tongs, or a red-hot poker, and moving it up and down close to the band.   The way in which the weight rises when the red-hot body is near, and falls when it is removed, is quite startling.   Joule experimented on the amount of shrinking per degree of elevation of temperature, with different weights hung on a band of vulcanized india-rubber, and found that they closely agreed with the amounts calculated by my theory from the heating effects of pull, and cooling effects of ceasing to pull, which he had observed in the same piece of india-rubber.   Joule's experiments leave the statements of the following paragraph (5) true for common india-rubber at 5° C., but reverse it for common india-rubber at higher temperatures and for vulcanized india-rubber—that is to say, leave it applicable to these substances with " pull " substituted for " push " throughout.]

(5) We may conclude as highly probable, that pushing a column of india-rubber together longitudinally while leaving it free at its sides will cause the evolution of heat, when the force by which its ends are pushed together falls short of a certain limit; but that, on the contrary, if this force exceeds a certain limit, cold will be produced by suddenly increasing the force a very little, so as to contract the column further. For I suppose it is certain that a column of india-rubber with no weight, or only a small weight on its top, will expand longitudinally when its temperature is raised; but it appears to me highly probable that if the weight on the top of the column exceed a certain limit, the diminished rigidity of the column will allow it to descend when the temperature is raised.   [This second change we now know to be contrary to the true state of the case ; for we have seen that the rigidity of india-rubber is augmented by elevation of temperature.]

22. The specific heat of an elastic solid homogeneously strained under given pressures or tensions will be obtained by finding the differential coefficients of $x, y, z, \xi, \eta, \zeta$ with reference to t, so as to make P, Q, R, S, T, U each remain constant or vary in a given manner—that is to say, by finding the coefficients of expansion in various dimensions for the body with an infinitely small change of its temperature, and using these in (3) above.

23. The elastic properties of such a crystal as is frequently found in natural specimens of garnet—a regular rhombic dodecahedron—must, if they correspond to the crystalline form, be symmetrical with reference to six axes in the substance perpendicular to the six pairs of opposite faces of the dodecahedron, or to the six edges of a regular tetrahedron related to

the dodecahedron in a determinate manner (having for its corners four of the eight trihedral corners of the dodecahedron); and yet they may differ, and in all probability they do differ, in different directions through the crystal. The relations among the coefficients of elasticity, according to the system of independent variables used in the preceding paper, which are required to express such circumstances, may be investigated by choosing for the normal cube a cube with faces perpendicular to the lines joining the three pairs of opposite tetrahedral corners of the dodecahedron. This choice of the normal cube makes all the coefficients vanish except nine, and makes these nine related one to another as follows :—

$$\left(\frac{d^2w}{dx^2}\right)_0 = \left(\frac{d^2w}{dy^2}\right)_0 = \left(\frac{d^2w}{dz^2}\right)_0 = \lambda + 2\mu,$$

$$\left(\frac{d^2w}{d\xi^2}\right)_0 = \left(\frac{d^2w}{d\eta^2}\right)_0 = \left(\frac{d^2w}{d\zeta^2}\right)_0 = \mu + \kappa,$$

and

$$\left(\frac{d^2w}{dy\,dz}\right)_0 = \left(\frac{d^2w}{dz\,dx}\right)_0 = \left(\frac{d^2w}{dx\,dy}\right)_0 = \lambda;$$

where λ, μ, κ are three independent coefficients, introduced merely for the sake of comparison with M. Lamé's notation. In different natural crystals of the cubical system, such as fluor-spar, garnet, &c., it is probable that the three coefficients here left have different relations with one another. The body would, as is known, be, in its elastic qualities, perfectly isotropic if, and not so unless, the further relation

$$\left(\frac{d^2w}{dx^2}\right)_0 = \left(\frac{d^2w}{dy\,dz}\right)_0 + 2\left(\frac{d^2w}{d\xi^2}\right)_0$$

were fulfilled. Hence the quantity κ in the preceding formulæ expresses the crystalline quality which I suppose to exist in the elasticity of a crystal of the cubic class.

24. The fact of there being six axes of symmetry in cubic crystals (diagonals of sides of the cube), has suggested to me a system of independent variables, symmetrical with respect to those axes, which I believe may be found extremely convenient in the treatment of a mechanical theory of crystallography, and which, so far as I know, has not hitherto been introduced for the expression of a state of strain in an elastic solid. It is simply the *six edges of a tetrahedron enclosing always the same part of the solid,* a system of variables which might be used in all expressions connected with the theory of the elasticity of solids. To apply it to express the elastic properties of a crystal of the cubical class, let the tetrahedron be chosen with its edges parallel

to the six lines which are lines of symmetry when the solid is unstrained. In any state of strain let x, y, z be the lengths of three edges lying in one plane, and ξ, η, ζ those of the three others (which meet in a point). Let x_0, y_0, z_0, ξ_0, η_0, ζ_0 denote the values (equal among themselves) of these variables for the unstrained state, and let w be the work required to bring any portion of the solid (whether the tetrahedron itself or not is of no consequence) from the unstrained state to the state (x, y, z, ξ, η, ζ) while kept at a constant temperature. The relations among the coefficients of elasticity according to this system of variables, to express perfect symmetry with reference to the six axes, will clearly be:—

$$\left(\frac{d^2w}{dx^2}\right)_0 = \left(\frac{d^2w}{dy^2}\right)_0 = \left(\frac{d^2w}{dz^2}\right)_0 = \left(\frac{d^2w}{d\xi^2}\right)_0 = \left(\frac{d^2w}{d\eta^2}\right)_0 = \left(\frac{d^2w}{d\zeta^2}\right)_0 = \varpi \, ;$$

$$\left(\frac{d^2w}{dy\,dz}\right)_0 = \left(\frac{d^2w}{dz\,dx}\right)_0 = \left(\frac{d^2w}{dx\,dy}\right)_0 = \left(\frac{d^2w}{d\eta\,d\zeta}\right)_0 = \left(\frac{d^2w}{d\zeta\,d\xi}\right)_0$$

$$= \left(\frac{d^2w}{d\xi\,d\eta}\right)_0 = \left(\frac{d^2w}{dx\,d\eta}\right)_0 = \left(\frac{d^2w}{dx\,d\zeta}\right)_0 = \left(\frac{d^2w}{dy\,d\zeta}\right)_0$$

$$= \left(\frac{d^2w}{dy\,d\xi}\right)_0 = \left(\frac{d^2w}{dz\,d\xi}\right)_0 = \left(\frac{d^2w}{d\zeta\,d\eta}\right)_0 = \sigma;$$

$$\left(\frac{d^2w}{dx\,d\xi}\right)_0 = \left(\frac{d^2w}{dy\,d\eta}\right)_0 = \left(\frac{d^2w}{dz\,d\zeta}\right)_0 = \omega, —$$

where ϖ, σ, ω denote three independent coefficients of elasticity for the substance. The definition of the new system of variables may be given as simply, and in some respects more conveniently, by referring to the dodecahedron, whose faces are perpendicular to the edges of the tetrahedron. Thus the six variables $x, y, z, \xi, \eta, \zeta$ may be taken to denote respectively the mutual distances of the six pairs of parallel faces of the rhombohedron into which the regular dodecahedron is altered when the solid is strained in any manner. Thus, if the portion of the solid considered be the dodecahedron itself, and of such dimensions that when it is in its normal state the area of each face is unity, the values of $\frac{dw}{dx}$ &c., denoted, as in the preceding paper, by P, Q, R, S, T, U, are normal tensions (reckoned, as usual, per unit of area), on surfaces in the solid parallel to the faces of the dodecahedron, which compounded give the actual straining force to which the solid is subjected. The coefficients denoted above by ϖ, σ, ω are such as to give the following expressions for the component straining tensions in terms of the strains :—

$$P = \varpi(x - x_0) + \omega(\xi - \xi_0) + \sigma(y - y_0 + z - z_0 + \eta - \eta_0 + \zeta - \zeta_0);$$
$$Q = \varpi(y - y_0) + \omega(\eta - \eta_0) + \sigma(z - z_0 + x - x_0 + \zeta - \zeta_0 + \xi - \xi_0);$$
$$R = \varpi(z - z_0) + \omega(\zeta - \zeta_0) + \sigma(x - x_0 + y - y_0 + \xi - \xi_0 + \eta - \eta_0);$$
$$S = \varpi(\xi - \xi_0) + \omega(x - x_0) + \sigma(y - y_0 + z - z_0 + \eta - \eta_0 + \zeta - \zeta_0);$$
$$T = \varpi(\eta - \eta_0) + \omega(y - y_0) + \sigma(z - z_0 + x - x_0 + \zeta - \zeta_0 + \xi - \xi_0);$$
$$U = \varpi(\zeta - \zeta_0) + \omega(z - z_0) + \sigma(x - x_0 + y - y_0 + \xi - \xi_0 + \eta - \eta_0).$$

25. The three quantities, ϖ, ω, σ, or the three coefficients of elasticity according to the new system of independent variables, will express, by their different relative values, the elastic properties of all crystals of the cubical class. For a perfectly isotropic body, a particular numerical relation, which I have not yet determined, must hold between ϖ, ρ, and σ; and two independent coefficients of elasticity will remain. To determine this relation, and to find the formulæ of transformation from one set of variables to another on the new system, or from the new system to the ordinary system (that which was used in the preceding portion of this paper), or *vice versâ*, may be interesting objects of inquiry.

Glasgow College, March 10, 1855.

26. *Extracted from Nichol's ' Cyclopædia of the Physical Sciences,'* second edition, 1860. *Thermomagnetism.* (1) *Experimental Facts.*—Gilbert found that if a piece of soft iron between the poles of a magnet be raised to a bright red heat it loses all its ordinary indications of magnetism, and it only retains (Faraday, 'Exp. Res.' 2344–2347) slight traces of the paramagnetic character. Nickel loses its magnetic inductive capacity very rapidly as its temperature rises about 635° Fahr., and has very little left at the temperature of boiling oil. Cobalt loses its inductive capacity at a far higher temperature than that of either, near the melting-point of copper. Of the three metals, iron remains nearly constant, nickel falls gradually, and *cobalt actually rises* in inductive capacity as the temperature is raised from 0° to 300° Fahr. (Faraday, 'Exp. Res.' 3428 ; 'Phil. Trans.' Nov. 1855). Cobalt, of course, must have a maximum inductive capacity at some temperature intermediate between 300° Fahr. and the temperature of melting copper. Crystals, when their temperatures are raised, have their magnetic inductive capacities in different directions of the crystalline substance rendered less unequal, and in general to a very marked degree. Thus Faraday found the difference of inductive capacities in different directions in a crystal of bismuth (a diamagnetic crystal) reduced to less than half when the temperature was raised from 100° to 280°.

In carbonate of iron (a paramagnetic crystal) the difference of inductive capacities in different directions was reduced to one third when the temperature was raised from 70° to 289° Fahr., and was tripled when the temperature was again brought down to 70° (Exp. Res. 3400 and 3411).

(2) *Thermodynamic Relations.*—The theory of the mutual convertibility of heat and mechanical work in reversible operations when applied to these phenomena proves :—1. That a piece of soft iron at a moderate or low red heat, when drawn gently away from a magnet experiences a cooling effect, and when allowed to approach a magnet experiences a heating effect; that nickel at ordinary temperatures, and cobalt at high temperatures, within some definite range below that of melting copper, experience the same kind of effects when subjected to similar magnetic operations. 2. That cobalt at ordinary atmospheric temperatures, and at all temperatures upwards to its temperature of maximum inductive capacity, experiences a cooling effect when allowed to approach a magnet slowly, and a heating effect when drawn away. 3. That a crystal in a magnetic field experiences a cooling effect when its axis of greatest paramagnetic or of least diamagnetic inductive capacity is turned round from a position along to a position across the lines of force, and a heating effect when such a motion is reversed.

[27. Let there be three rectangular axes fixed relatively to the movable body, whether soft iron, or copper, or a crystal in a magnetic field, and, considering the whole magnetic motive [*] on the body, reduce it, after the manner of Poinsot, to three component forces along the magnetic axes and three couples round these axes. Let P, Q, R be the force-components, and S, T, U the couple-components thus obtained, which we must suppose to be known functions of t, the temperature. Equation (22) of § 20 above gives H, the quantity of heat which must be supplied to prevent the body from becoming cooler when it is moved through infinitesimal spaces $x - x_0$, $y - y_0$, $z - z_0$ in the directions of the three axes, and turned through infinitesimal angles $\xi - \xi_0$, $\eta - \eta_0$, $\zeta - \zeta_0$ round the same axes. The lowering of temperature which it experiences if heat is neither given to it nor taken from it is equal to $\dfrac{H}{C}$, where C denotes the whole capacity for heat of the body, or the product of its mass or bulk by its specific heat per unit of

[*] [In dynamics the want is keenly felt of an expression for a system of forces acting on a body : adopting a suggestion of my brother, Professor James Thomson, the word "motive" is used in the text to supply this want.]

mass or per unit of bulk. If the directions of x, y, z and ξ, η, ζ are such that P, Q, R, S, T, U are positive, then for for iron and nickel, and for cobalt at temperatures above that of its maximum inductive capacity,

$$-\frac{d\mathrm{P}}{dt}, \quad -\frac{d\mathrm{Q}}{dt}, \quad -\frac{d\mathrm{R}}{dt}, \quad -\frac{d\mathrm{S}}{dt}, \quad -\frac{d\mathrm{T}}{dt}, \quad -\frac{d\mathrm{U}}{dt}$$

are positive, and therefore the substance experiences a cooling effect when it is moved in such a manner as to require work to be done against magnetic force; and the reverse is the case for cobalt at ordinary temperatures.]

28. *Extracted from Nichol's 'Cyclopædia of the Physical Sciences,'* second edition, 1860.—The most probable account that can be given of the pyroelectric quality of dipolar crystals is, that these bodies intrinsically possess the same kind of *bodily electro-polarization* which Faraday, in his 'Experimental Researches,' has clearly proved to be temporarily produced in solid and liquid nonconductors, and that they possess this property to different degrees at different temperatures.

The inductive action exercised by this electro-polar state of the substance, on the matter touching the body all round, induces a superficial electrification which perfectly balances its electric force on all points in the external matter; but when the crystal is broken in two across its electric axis, the two parts exhibit as wholes contrary electrifications, not only by the free electro-polarities on the fractured surfaces discovered by Canton, but by the induced electrification on the old surface, belonging to the old state of electric equilibrium, and gradually lost by slow conduction, while a new superficial distribution of electricity on each fragment is acquired which ultimately masks all external symptoms of electric excitement. When the temperature of the substance is changed, its electro-polarization changes simultaneously, while the masking superficial electrification follows the change only by slow degrees— more or less slow according to the greater or less resistance offered to electric conduction in the substance or along its surface.

[29. If the preceding explanation of pyroelectricity be true, it must follow that a pyroelectric crystal moved about in an electric field will experience cooling effects or heating effects calculable by formula (22) of § 20, with the same notation for the electric subject as that of § 27 for magnetism. Thus the effects will be the same for a crystal at the same temperature whatever be the electrification of its surface. Thus it is remarkable that, in virtue of the wholly latent electric

polarity of a seemingly neutral pyroelectric crystal (that is to say, a crystal at the surface of which there is an electrification neutralizing for external space the force due to its internal electric polarity), the same cooling and heating effects will be produced by moving it in an electric field, as similar motions would produce in a similar crystal which, by having been heated in hot water, dried at the high temperature, and cooled, is in a state of pyroelectric excitement.]

Yacht 'Lalla Rookh,'
Largs, Sept. 13, 1877.

III. *On Binocular Vision*

To the Editors of the Philosophical Magazine and Journal.

Gentlemen,

IN Mr. Thompson's excellent article on the Chromatism of the Eye, in the July Number of your Journal, I notice the following sentence :—" In Wheatstone's classical research of 1838 it was demonstrated how great is the capacity of the brain to combine two slightly differing retinal images." From this sentence I conclude that Wheatstone's theory of binocular relief is still held by many scientists : I am confirmed in this conclusion by finding the expression " *mental fusion* " or " *conscious fusion* " of dissimilar images in nearly all works on the subject, even the latest, viz. Hermann's ' Physiology.' Now, as I am quite sure that this theory is not only untenable, but positively hurtful to science by discouraging that careful analysis of visual perception so necessary to a true theory and yet so difficult to most persons, I have thought it would not be amiss to state briefly what I conceive to be the present condition of science on this subject.

In all investigations on binocular vision we are met at the very threshold by the difficulty which most persons find in analyzing what I would call *visual judgments,* i. e. judgments which by long habit and inherited tendency seem at first to be direct sensuous perceptions incapable of further analysis. It is difficult to convince *many* persons that they ever see double images at all ; and yet they, of course, every day form judgments based on the existence and the unconscious perception of such double images. It is difficult to convince *most* persons, even the thoughtfully observant, that in regarding a stereoscopic scene there is no complete fusion of the two pictures, but that, when the eye is fixed on the foreground, objects in the background are double, and *vice*

versâ. It is still more difficult, nay, almost impossible, even for those accustomed to analyze these visual perceptions, to perceive a similar partial doubling of images in regarding *an actual solid object.* Yet by careful analysis we may convince ourselves of all this. Nothing can be more certain than the fact that the complete fusion of dissimilar images never takes place, and that, if we think otherwise, it is only because we do not observe and analyze carefully.

From early boyhood I have accustomed myself to make experiments on binocular vision, and have thus gradually acquired an extraordinary aptitude in decomposing complex visual judgments into their component sense-impressions. In combinations of stereoscopic pictures, whether in the stereoscope or with the naked eye by squinting (*i. e.* whether beyond or on this side the plane of the pictures, and therefore whether the binocular relief be *natural* or *inverted*), I always distinctly perceive the doubling of parts of the scene or object when a nearer or a more distant part is regarded. Also in viewing natural objects, even such objects as small cones or prisms, I always perceive the doubling of the nearer parts while regarding the further parts, and *vice versâ.* Wheatstone's theory therefore seems true only to the unobservant or unpractised: it is a popular explanation, not a scientific theory ; it cuts, but does not loose the Gordian knot.

Brücke and Brewster, by more refined observation and more careful analysis, easily perceived that there was in reality no mental fusion of two dissimilar images. Their view is that, in regarding a solid object or two stereoscopic pictures, the eyes are in incessant unconscious motion, by greater and less optic convergence successively combining the different parts of the two images, and thus by ranging back and forth reach by trial the distinct perception of binocular relief.

This theory is undoubtedly a great advance upon Wheatstone's. It is really a scientific theory, since it is based upon an analysis of our visual judgment. It is also in part a true theory : but it is evidently not the whole truth ; for successive trial-combinations of different parts of the two images are not necessary to the perception of binocular perspective. The experiments of Dove have proved that binocular relief is distinctly perceived even by the light of an electric spark, which lasts only $\frac{1}{24000}$ of a second—a time too short to allow any change of optic convergence.

I have repeated these experiments of Dove with great care, with the double object of testing their accuracy and at the same time of testing the truth of a view which I had pre-

viously formed on this subject. My experiments completely confirmed the results of Dove. I found that binocular relief, both by combination of stereoscopic diagrams (whether in the stereoscope or by the naked eye) and by viewing natural objects, is indeed perfectly distinct by the light of the electric spark; but I also observed in all cases the doubling of the nearer lines or objects while regarding the more remote, and *vice versâ*.

Between the two rival theories, then, the matter stands thus:—Wheatstone is *right* in so far as he asserts *immediate perception* of relief, but is *wrong* in supposing any *mental fusion* of the two images. Brücke is *right* in asserting that the perception of binocular relief is a judgment based upon *double images* of all parts of the object or scene beyond or on this side the point of sight, but is *wrong* in supposing that *change of optic convergence and successive trial combination* is a necessary part of the evidence on which judgment is based. My own view, or theory if I may so call it, has already been published[*]. It is an attempt to unite what is true in the two preceding views. I quote from a previous paper :—" All objects or points either beyond or on this side the point of sight are doubled, but differently—the former *homonymously*, the latter *heteronymously*; the double images of the former are united by *less*, of the latter by *greater* convergence. Now the observer knows *instinctively and without trial*, in any case of double images *whether they will be united by greater or by less convergence*, and therefore never makes a mistake, or attempts to unite by a wrong movement of the optic axes. In other words, the eye (or the mind) *instinctively distinguishes between homonymous and heteronymous images, referring the former to objects or points beyond, and the latter to objects or points on this side the point of sight*. The mind therefore perceives relief *instantly* by means of double images in the manner just explained; but the perception is doubtless made clearer by changes of optic convergence, by ranging the eyes back and forth from foreground to background and *vice versâ*, and the successive combination of different parts of the object or pictures, as maintained by Brücke."

<div style="text-align:center">I am, Gentlemen,
Yours very respectfully,
JOSEPH LE CONTE.</div>

Berkeley, California,
November 17, 1877.

* Amer. Journ. vol. ii. pp. 1, 315, 417; *Archives des Sciences,* vol. xli. p. 394 (1871).

IV. *Electromagnetic and Calometric Absolute Measurements:*
the Absolute Value of Siemens's Unit of Resistance in Electro-
magnetic Measure; the Relation between the Current-work
and the Heat-evolution in stationary Galvanic Currents; and
the Absolute Values of some constant Hydroelectromotive
Forces in Electromagnetic Measure. (Condensed Comparison
of the Results of a Series of Investigations.)   By H. F.
WEBER, *Professor of Mathematical and Technical Physics*
at the Federal Polytechnic Academy of Zurich *.

SINCE Siemens's unit of resistance has been admitted into
the department of galvanic measurements, the attempt has
been made in four different quarters to fix the absolute value
of this empiric unit—that is, to determine in absolute measure
that electromotive force which, in a conductor whose resistance
is equal to that of Siemens's unit, is capable of calling forth a
current of absolute intensity.

1. The fundamental system of measurement was the elec-
tromagnetic.

In 1862, W. Weber, according to a method devised by
himself (*Abhandlungen der Göttinger Gesellschaft,* Band x.),
found as the absolute value of the Siemens mercury unit :—

$$1 \text{ S. M. U.} = 1 \cdot 0257 \times 10^{10} \left(\frac{\text{millim.}}{\text{sec.}} \right).$$

According to the same method, and by aid of the same in-
struments, F. Kohlrausch †, eight years later, repeated the
determination, and, from four different measurements, obtained
as the mean value

$$1 \text{ S. M. U.} = 0 \cdot 9717 \times 10^{10} \left(\frac{\text{millim.}}{\text{sec.}} \right).$$

The committee appointed by the British Association for the
Advancement of Science to determine upon a suitable unit of
resistance, consisting of Messrs. Clerk Maxwell, Balfour
Stewart, and Jenkin, in the course of the years 1863 and 1864
produced a resistance, the British-Association unit (by English
physicists called also the " ohm "), which is said to represent
in electromagnetic measure exactly the absolute value 10^{10}
$\left(\frac{\text{millim.}}{\text{sec.}} \right).$ According to the best comparisons, this British
unit is to the Siemens as $1 : 0 \cdot 9536$; consequently, according
to the measurements of the English physicists, the absolute

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* Translated from the German original communicated by the Author.
† Pogg. *Ann.* Erg.-Band vi. p. 1.

value of Siemens's unit would be equal to $0 \cdot 9536 \times 10^{10}$ $\left(\dfrac{\text{millim.}}{\text{sec.}} \right)$.

More recently M. Lorenz, in Copenhagen, by a very simple method peculiar to himself[*], in which induced currents of *constant* strength were employed, has measured the magnitude of the Siemens unit of resistance in absolute electromagnetic measure, obtaining as the final result of his measurements :—

$$1 \text{ S. M. U.} = 0 \cdot 9333 \times 10^{10} \left(\dfrac{\text{millim.}}{\text{sec.}} \right).$$

How many different observers have determined the absolute quantity of the Siemens resistance-unit, so many *different*, indeed *very different*, results have been found. With the delicacy now attained of galvanometric methods of observation, with the completeness with which we believe we understand the fundamental laws of current-electricity, certainly no one anticipated that in the final results of physicists so practised in this kind of work there could appear so *great* a divergency. These four different results, when compared, present a new problem, and one of fundamental importance for galvano-metry. The two *à priori* equally possible solutions of the problem are :—

(*a*) The four observers, or groups of observers, have carried out the difficult observations requisite for a determination of the absolute resistance without error ; and the final results differ because the natural laws assumed as the basis of the different methods of observation are not precisely the true ones. Or

(*b*) The natural laws employed are rigorously correct, and at least three of the above observers have committed some error.

In the following investigations it is found that the latter solution is the true one. Three essentially different methods, which brought into application three quite different natural laws, and in which both rapidly and slowly varying induced currents and also stationary flows came into use, have given a perfectly accordant final result for the absolute value of the Siemens resistance-unit—namely,

$$1 \text{ S. M. U.} = 0 \cdot 9550 \times 10^{10} \left(\dfrac{\text{millim.}}{\text{sec.}} \right) ;$$

and, besides, this result agrees, except an extremely slight difference, with the value obtained by the English physicists. Since, even with manifold variation of my three methods of

* Pogg. *Ann.* vol. cxlix. p. 251 (1873).

experiment, I was unable to effect any material change in my
final result, I am compelled to see, in the divergent results of
MM. Wilh. Weber, F. Kohlrausch, and L. Lorenz (who more-
over conducted the investigation each according to *one* method
only), values affected with errors of observation.

I. *Determination of the Absolute Value of the Siemens Resist-
ance-Unit on the basis of the Laws of Magneto-Induction.*

As my first method of experiment for the determination of
the value of the unit in question I chose a procedure which
had already been employed by Wilhelm Weber on the intro-
duction of absolute measurements of resistance [*] ; and I
managed it so that it could be carried out under two different
conditions.

Two exactly equal, extremely regularly wound cylindrical
spirals were connected with a multiplier so that their axes fell
into one and the same horizontal straight line, which was per-
pendicular to the magnetic meridian. The inner radius of
the spirals was 144·43 millims. ; the outer radius amounted to
184·46 ; the depth of the space occupied by the turns amounted
consequently to 40·03 millims. ; its breadth was 53·64 millims. ;
and each spiral numbered 691 turns. A most powerful paral-
lelepipedal magnet (its length, breadth, and height were 80,
20·1, and 21·1 millims. respectively) was placed with its centre
exactly in the axis of the two spirals, and as nearly as possible
in the middle between the central planes of the latter ; it was
supported by a thin brass wire of about 3 metres length. The
stated dimensions of the multiplier and the magnet are of such
a magnitude that, in the calculation of the mutual action
between multiplier and magnet, in the place of the latter a
system of two magnetic poles of equal magnetic moment could
be put.

If a magnet within a multiplier be rotated a small angle
from its position of equilibrium and left to the forces acting
upon it, it will describe isochronous oscillations, the amplitudes
of which diminish in a geometrical progression. With the
multiplier " open "
the oscillation-period

$$T_1 = \frac{\pi}{\sqrt{\dfrac{MH}{K} + \dfrac{B}{K} - \left(\dfrac{A}{2K}\right)^2}},$$

and the logarithmic decrement of the amplitudes

$$\lambda_1 = \frac{A}{2K} \cdot T_1.$$

$$\left. \begin{array}{c} \\ \\ \\ \\ \\ \\ \\ \end{array} \right\} \quad . \quad . \quad (1)$$

* *Electrodynamische Maassbestimmungen,* p. 232.

According to the law of magneto-induction, with the multiplier " closed "

the oscillation-period

$$T_2 = \cfrac{\pi}{\sqrt{\cfrac{MH}{K} + \cfrac{B}{K}\left(\cfrac{M^2G^2}{2Kw} + \cfrac{A}{2K}\right)^2}}, \quad . \quad . \quad (2)$$

and the logarithmic decrement of the amplitudes

$$\lambda_2 = \left(\frac{M^2G^2}{2Kw} + \frac{A}{2K}\right).T_2.$$

In these equations, K denotes the moment of inertia, and M the magnetic moment, of the magnet ; H the horizontal component of the earth's magnetic force ; B the torsion-moment of the suspension-wire ; A the rotation-moment with which the wire and the surrounding medium act upon the magnet moved with the angular velocity 1 ; G the electromagnetic force with which the multiplier, when the current 1 flows through it, acts on the magnetic unit of mass concentrated in one polar point ; and w the absolute value of the resistance of the multiplier (in electromagnetic measure).

From equations (1) and (2) result the further equations

$$\frac{\lambda_2}{T^2} - \frac{\lambda_1}{T_1} = \frac{M^2G^2}{2Kw},$$

and

$$\frac{\pi^2 + \lambda_1^2}{T_1^2} = \frac{\pi^2 + \lambda_2^2}{T_2^2};$$

and from these we get, for the absolute resistance w the expression

$$w = \frac{G^2M^2T_1}{2K\left[\lambda_2\sqrt{\dfrac{\pi^2 + \lambda_1^2}{\pi^2 + \lambda_2^2}} - \lambda_1\right]},$$

which, according to equation (1), can be replaced by

$$w = G^2\left(\frac{M}{H}\right)\frac{1}{2T_1.(1+\theta)}.\cfrac{\pi^2 + \lambda_1^2}{\lambda_2.\sqrt{\cfrac{\pi^2 + \lambda_1^2}{\pi^2 + \lambda_2^2}} - \lambda_1}, \quad . \quad (3)$$

where θ denotes the quantity $\dfrac{B}{MH}$. If the resistance of the multiplier has been found equal to n Siemens mercury units,

then the absolute value of one Siemens mercury unit (1 S. M. U)
in electromagnetic measure is :—

$$1 \text{ S. M. U.} = \frac{G^2}{n} \cdot \frac{M}{H} \cdot \frac{1}{2T_1(1+\theta)} \cdot \frac{\pi^2 + \lambda_1^2}{\lambda_2 \sqrt{\frac{\pi^2 + \lambda_1^2}{\pi^2 + \lambda_2^2} - \lambda_1}} \quad . \quad (4)$$

Strictly, in the development of the absolute value for w
account would also have to be taken of the fact that the cur-
rent induced primarily by the movement of the magnet is
variable with the time, and in consequence of this acts indu-
cingly on its own path. The carrying-out of the calculation
shows that the influence of this induction of the induced cur-
rent is so small in comparison with the other conditioning
moments, that the expression above given for w is, in conse-
quence thereof, only increased by (in round numbers) $\frac{1}{20000}$.
Since, in the measurements cited below, none of the quantities
to be determined could be measured with such accuracy that
an additional $\frac{1}{20000}$ of its value could have been safely esti-
mated, the influence of the induction on the part of the pri-
marily induced variable current might be completely ignored.

For the determination of the absolute value of the S. M. U.
by means of this procedure there are consequently *seven* dif-
ferent quantities to be measured.

The five quantities λ_1, λ_2, T_1, $(1 + \theta)$, and $\left(\frac{M}{H}\right)$ were deter-

mined by the method introduced by Gauss. The value of G
was calculated, by means of the fundamental law of electro-
magnetic action at a distance, from the dimensions and form
of the multiplier :

$$G = \frac{2\pi n R^2}{\rho^3} \left\{ \begin{array}{l} 1 + \frac{h^2}{R^2} \left\{ \frac{1}{3} - \frac{5}{2} \frac{R^2}{\rho^2} \left(1 - \frac{R^2}{\rho^2} \right) \right\} - \frac{b^2}{\rho^2} \left(\frac{1}{2} - \frac{5}{2} \frac{D^2}{\rho^2} \right) \\ - \frac{3}{4} \frac{l^2}{\rho^2} \left[\frac{4D^2 - R^2}{\rho^2} - \frac{h^2}{\rho^2} \left\{ \frac{5}{3} - \frac{14}{3} \frac{R^2}{\rho^2} + \frac{4D^2 - R^2}{\rho^2} \left(\frac{21}{6} + \frac{21}{2} \frac{D^2}{\rho^2} \right) \right\} \\ + \frac{b^2}{\rho^2} \left\{ \frac{4}{3} - \frac{56}{3} \frac{D^2}{\rho^2} - \frac{4D^2 - R^2}{\rho^2} \left(\frac{7}{6} - \frac{21}{2} \frac{D^2}{\rho^2} \right) \right\} \right] + \dots \end{array} \right.$$

Here n denotes the number of the turns of the multiplier,
R the mean radius of the turns, 2D the distance between
the central planes of the two spirals, $2h$ the height, and $2b$
the breadth, of the cross section of the space occupied by
the turns, ρ the quantity $\sqrt{R^2 + D^2}$, and $2l$ the distance be-
tween the poles of the oscillating magnet.

In deriving this expression it was presupposed that in place of the spiral turns circular turns might be put continuously filling the space occupied by the multiplier; further, the angle u of the deflection of the magnet was taken as so small that one might put $\cos u = 1$ and $5 \sin^2 u$ vanishingly small in comparison with 1. In the observations carried out u never exceeded the value 2°. The cylindrical spirals were so constructed and set up that the lengths R, D, h, and b could be accurately measured to within 0·1 millim. directly with the cathetometer.

The number n of the Siemens units which represented the resistance of the multiplier at the time of each observation was determined by aid of a bridge arrangement, which most carefully excluded all errors that might happen from extra currents, variations of temperature, dissimilar positions of the measuring-wire, the presence of transitory resistances, &c.

Eighteen series of experiments were carried out, according to this process, on 18 different days. The following was always the order of the operations:—determination of the number n; ascertaining of $\left(\dfrac{M}{H}\right)$ and l; then determination of the values T_1, λ_1, λ_2 from twelve successive series of observations with the multiplier alternately "open" and "closed;" and, lastly, repetition of the measurement of $\left(\dfrac{M}{H}\right)$, l, and n. The temperature of the observation-room never varied during any one series of experiments more than 0°·6 at the most, and was of course closely followed.

In order to get some light upon the trustworthiness of the results obtained by this method, two groups of experiments were instituted. In the *first* group the two spirals were pushed as near together as the suspension-wire of the magnet permitted (to the distance D = 39·2 millims.); with this the difference $\lambda_2 - \lambda_1$ proved to be, on the average, 0·0296. At the same time the term

$$-\frac{3}{4}\frac{l^2}{\rho^2}\left[\frac{4D^2-R^2}{\rho^2} - \frac{h^2}{\rho^2}\left\{\frac{5}{3} - \frac{14}{3}\frac{R^2}{\rho^2} + \frac{4D^2-R^2}{\rho^2}\left(\frac{21}{6} + \frac{21}{2}\frac{R^2}{\rho^2}\right)\right\}\right.$$
$$\left. + \frac{b^2}{\rho^2}\left\{\frac{4}{3} - \frac{56}{3}\frac{D^2}{\rho^2} - \frac{4D^2-R^2}{\rho^2}\left(\frac{7}{6} - \frac{21}{2}\frac{D^2}{\rho^2}\right)\right\}\right]$$

in the above-given general expression for G had here a value (about 2 per cent.) which together with the initial term 1 added considerably to its importance.

There were found :—

April 4,1876, 1 S. M. U. $= 0.9551 \times 10^{10} \left(\dfrac{\text{millim.}}{\text{sec.}} \right).$

,,	5,	,,	$= 0.9532 \times 10^{10}$	,,
,,	6,	,,	$= 0.9570 \times 10^{10}$	,,
,,	7,	,,	$= 0.9565 \times 10^{10}$	,,
,,	8,	,,	$= 0.9548 \times 10^{10}$	,,
,,	10,	,,	$= 0.9555 \times 10^{10}$	,,

The mean value of these six series is

$$1 \text{ S. M. U.} = 0.95535 \times 10^{10}.$$

In the *second* group of trials the spirals were pushed so far asunder that *the distance between their central planes was as closely as possible equal to the mean radius of their windings.* For this position of the spirals ($2D = 164.4$ millims. nearly) the difference of the logarithmic decrements amounted to only about 0.0172 ; at the same time the expression of G was *approximately independent* of the pole-distance of the magnet: for the case that $D = \dfrac{R}{2}$,

$$G = \frac{16\pi \cdot n}{5\sqrt{5} \cdot R} \left[1 - \frac{1}{15} \frac{h^2}{R^2} + \frac{3}{4} \frac{l^2}{\rho^2} \left(\frac{36}{15} \frac{b^2}{\rho^2} - \frac{31}{15} \frac{h^2}{\rho^2} \right) \right];$$

and the value of the last member within the square brackets amounts to only -0.00028.

The results found with this arrangement of the experiments were :—

April 12, 1876, 1 S. M. U. $= 0.9531 \times 10^{10} \left(\dfrac{\text{millim.}}{\text{sec.}} \right).$

,,	13,	,,	$= 0.9543 \times 10^{10}$	,,
,,	14,	,,	$= 0.9542 \times 10^{10}$	,,
,,	15,	,,	$= 0.9534 \times 10^{10}$	,,
,,	16,	,,	$= 0.9555 \times 10^{10}$	,,
,,	17,	,,	$= 0.9528 \times 10^{10}$	,,

The mean value amounts to

$$1 \text{ S. M. U.} = 0.95388 \times 10^{10} \left(\frac{\text{millim.}}{\text{sec.}} \right).$$

During the summer of 1876 the multiplier was taken to pieces ; in the autumn I once more subjected all the dimensions of both spirals to a cathetometric examination, and again put the spirals together so as to form a multiplier of the sort last described. The moment of the magnet had, in consequence of continual use at different times, become so considerably diminished that the difference of the logarithmic decrements $\lambda_2 - \lambda_1$ now amounted to only about 0.0161.

The results found in this third series were :—

Sept. 15, 1876, 1 S. M. U. $= 0{\cdot}9551 \times 10^{10} \left(\dfrac{\text{millim.}}{\text{sec.}} \right)$.

$\quad$,, $\quad$ 16, ,, $\qquad\qquad\qquad = 0{\cdot}9550 \times 10^{10} \quad$,,

$\quad$,, $\quad$ 17, ,, $\qquad\qquad\qquad = 0{\cdot}9548 \times 10^{10} \quad$,,

$\quad$,, $\quad$ 18, ,, $\qquad\qquad\qquad = 0{\cdot}9527 \times 10^{10} \quad$,,

$\quad$,, $\quad$ 19, ,, $\qquad\qquad\qquad = 0{\cdot}9538 \times 10^{10} \quad$,,

$\quad$,, $\quad$ 20, ,, $\qquad\qquad\qquad = 0{\cdot}9544 \times 10^{10} \quad$,,

According to these, in the mean,

$$1 \text{ S. M. U.} = 0{\cdot}95430 \times 10^{10} \left(\frac{\text{millim.}}{\text{sec.}} \right).$$

The total result of all the measurements is :—The absolute value of the Siemens resistance-unit, in electromagnetic measure, derived from the electromotive forces and the galvanic currents which are induced by slow oscillating movements of a magnet in a linear conductor in its vicinity is, in the mean from eighteen series of trials, $0{\cdot}95451 \times 10^{10} \left(\dfrac{\text{millim.}}{\text{sec.}} \right)$.

II. *Ascertainment of the Absolute Value of the Siemens Mercury Unit by aid of the Laws of Voltaic Induction.*

Notwithstanding the perfectly satisfactory accordance of the individual results of the experiments decribed in section I., I have yet derived the absolute value of Siemens's empiric resistance-unit by a second, *essentially* different method. While in the first method the laws of *magneto-induction*, produced by *slow motion* of a magnet were employed, in the second the laws of *voltaic induction*, generated by *rapidly varying* galvanic currents, were used.

The two large cylindrical spirals which in the previous experiments had served as multiplier, were in these new experiments set up so that their axes fell into one and the same straight line, and their middle planes had a certain distance D. One of the spirals, the inducing, together with a simple circular ring of 165·7 millims. radius, was inserted in the circuit of a Daniell's pile, which was so constructed that it furnished for hours an almost absolutely constant current. The other, the induced spiral, formed with a third large cylindrical spiral of 370 windings a closed circuit. The last spiral was composed of two exactly equal halves, separated by a narrow interval. The radius of the innermost turn of this spiral was 154·2 millims., that of the outermost 172·22 millims. The space occupied by the windings had a rectangular cross section of the breadth of 33·5 millims. ; the central planes of the two halves were distant from each other 20·75 millims. Exactly in the middle of the interval separating the two halves was placed the above-mentioned circular ring of the radius 165·7

millims. ; its plane was parallel to the turns of the spiral ; its centre was on the axis of the latter. A small magnet of 40 millims. length was suspended, by a single cocoon-thread, exactly in the middle of the spiral.

The following was the method of experiment :—The induced circuit being open, a constant current was produced in the inducing circuit, the intensity of which, I, was measured in absolute measure by the action of the ring upon the little magnet. Then the inducing circuit was opened, the magnet brought to rest, the ring taken out of the inducing circuit and the latter again closed. After the path of the induced current was also closed, the inducing current I was opened ; the induction-current called forth by the sudden sinking of the intensity of the inducing-current to zero was measured by its integral current. Hereupon the inducing current's intensity I was again determined, and so on. Thus were taken from 20 to 30 successive measurements of the inducing current's intensity I and of the integral current j, generated by opening-induction. In none of the series of experiments carried out did the intensity I vary, in the course of from one to two hours, more than about $\frac{1}{2}$ per cent.

The calculation of the induction-processes thus excited was based upon the following assumptions :—

(1) The course of the induction produced by sudden alteration of current-intensity in the inducing circuit is perfectly represented by the general law of induction set forth by F. E. Neumann ; and

(2) The induced current called forth by this extremely rapidly passing induction fulfils Ohm's law.

Mr. F. E. Neumann, in his treatise *Die mathematischen Gesetze der inducirten electrischen Ströme*, had not more closely investigated this kind of induction. He says, "So far as these formulæ admit of being applied to those cases in which a galvanic current suddenly appears or is interrupted, further experimental trial is required ; for they presuppose that the velocity with which the inducing cause enters is inconsiderable in comparison with the velocity of propagation of electricity in an induced conductor. On the assumption of the applicability of formulæ (16) and (17) to the induction occasioned by the sudden rise or disappearance of galvanic currents, we can say that the current induced in a conductor at rest by the sudden appearance of a galvanic current is the same as if the conductor had moved towards the current, from infinite distance to the place where it is." That currents induced by swiftly-passing fluctuations of a current actually range themselves under Neumann's general law of induction, and at the same time indeed follow Ohm's law, Helmholtz (in his memoir

on the duration and course of electrical currents induced by current-intermissions) showed, some years later, by a series of measurements. Since the question whether the induced currents originated by sudden current-intermissions exactly follow Ohm's law or not, cannot be theoretically decided universally, but only be answered empirically in each individual case, I have, in order to gain a perfectly sure foundation for the measurements attempted, first instituted, in a preliminary investigation, as severe a trial as possible of how far the currents induced by sudden opening of the inducing circuit, in my arrangement of the experiments, follow Ohm's law. In this preliminary investigation nothing could be perceived that would intimate that induced currents arising from sudden current-variations do not exactly follow Ohm's law.

If I_0 denotes the current-intensity whose sudden diminution to zero effects the induction, P the mutual electrodynamic potential of the two spirals, if i represents the induced-current intensity present at the moment t of the induction-process, and w the resistance of the induced circuit, then is the equation

$$w \int_0^{t_1} i \cdot dt = w \cdot j = P \cdot I_0. \quad \ldots \quad (1)$$

(if we suppose that the induction commences at the moment $t=0$ and has already finished at the moment $t=t_1$) the resulting expression which is gained as soon as Neumann's general law of induction and Ohm's law are applied to the process of " opening "-induction. The absolute measurement of w in electromagnetic measure was carried out according to this equation (1).

The electromagnetic potential of the two spirals has the value

$$P = \iint \frac{ds_1 \cdot ds_2}{r} \cos v,$$

where ds_1 denotes any linear element of the one spiral, ds_2 any element of the other spiral, r the distance between these elements, and v the angle which their directions make with one another, and where the integration has to be extended over all the elements of both spirals. Into the somewhat lengthy working-out of the calculation of the quantity P we do not, in this abstract, enter further.

The absolute electromagnetic value of the current-intensity I_0 is obtained from the deflection-angle u, measured by aid of mirror, scale, and telescope :

$$I_0 = \frac{1}{2\pi} \cdot R \cdot H \left(1 + \frac{B}{MH}\right) \left(1 - \frac{3}{4} \frac{l^2}{R^2}\right) \cdot \tan u,$$

where B denotes the moment of torsion of the cocoon-thread, M the magnetic moment, and $2l$ the distance of the pole-points of the small magnet.

If we call T the duration of an oscillation of the small magnet, λ the logarithmic decrement of the amplitudes of the oscillating magnet in the closed multiplier, G the electromagnetic force with which the multiplier, passed through by the current 1, acts upon the magnetic unit of mass $(+1)$ present in one pole-point of the magnet, and, lastly, a the arc which the magnet describes from its position of rest in consequence of the action of the induced integral current j, then the absolute electromagnetic measure of the integral current generated

$$j = \frac{H}{G} \cdot \frac{a}{\pi} \cdot T\left(1 + \frac{B}{MH}\right) \cdot e^{\frac{\lambda}{2}}.$$

According to this, we have for the absolute value of w :—

$$w = P \cdot \frac{R \cdot G\left(1 - \frac{3}{4}\frac{l^2}{R^2}\right)}{2T \cdot e^{\frac{\lambda}{2}}} \cdot \frac{\tan u}{a}.$$

For the multiplier used, G had the value

$$G = \frac{2\pi \cdot n \cdot r^2}{\rho^3} \left\{ \begin{array}{l} 1 + \frac{h^2}{r^2}\left\{\frac{1}{3} - \frac{5}{2}\frac{r^2}{\rho^2}\left(1 - \frac{r^2}{\rho^2}\right) - \frac{b}{\rho^2}\left(\frac{1}{2} - \frac{5}{2}\frac{D^2}{\rho^2}\right)\right. \\ -\frac{3}{4}\frac{l^2}{\rho^2}\left[\frac{4D^2 - r^2}{\rho^2} - \frac{h^2}{\rho^2}\left[\frac{5}{3} - \frac{14}{3}\frac{r^2}{\rho^2} + \frac{4D^2 - r^2}{\rho^2}\left(\frac{21}{6} + \frac{21}{2}\frac{D^2}{\rho^2}\right)\right]\right] \\ \left. + \frac{b^2}{\rho^2}\left\{\frac{4}{3} - \frac{56}{3}\frac{D^2}{\rho^2} - \frac{4D^2 - r^2}{\rho^2}\left(\frac{7}{6} - \frac{21}{2}\frac{D^2}{\rho^2}\right)\right\}\right] + \dots \end{array} \right\};$$

and there were

$$n = 370 \qquad\qquad 2h = 18\cdot0 \text{ millims.}$$
$$r = 163\cdot2 \text{ millims.} \qquad 2b = 33\cdot5 \quad \text{,,}$$
$$\rho = 164\cdot5 \quad \text{,,} \qquad\qquad 2l = 33\cdot0 \quad \text{,,}$$
$$D = 20\cdot7 \quad \text{,,}$$

To find the value of the Siemens unit of resistance in absolute measure, two ways of proceeding were adopted :—

(1) The resistance w was measured in Siemens mercury units by the bridge method. It was found that w was equal to m Siemens units. Thus the absolute value of

$$1 \text{ S. M. U} = \frac{P.R.G.\left(1 - \frac{3}{4}\frac{l^2}{R^2}\right)}{m.T.e^{\frac{\lambda}{2}}}\frac{\tan u}{a}$$

(2) A completely stoppled Siemens stopple-rheostat was inserted in the induced circuit, and first the arc a determined which was given as deflection with the total resistance w of the induced circuit; upon this, without making any change in P, R, G, u, &c., 10 S. M. U. of the rheostat were inserted in the induced circuit in addition to w. If then the arc of deflection was a_1, the absolute value of

$$10 \text{ S. M. U.} = \frac{\text{P . R . G}\left(1 - \frac{3}{4}\frac{l^2}{\text{R}^2}\right)\tan u}{2\text{T} . e^{\frac{\lambda}{2}}}\left(\frac{1}{a_1} - \frac{1}{a}\right).$$

According to each of these two proceedings two series of observations were made—the one with the employment of a very great potential-value P and a moderate intensity I_0 of the inducing current, the other with the employment of a comparatively small potential-value P and an extremely great intensity I_0 of the inducing current. As absolute value of the Siemens unit there was found :—

<div align="center">

Series I.
P large, I_0 moderate.
Method 1.

</div>

Aug. 20, 1876, $0\cdot9558 \times 10^{10}\left(\dfrac{\text{millim.}}{\text{sec.}}\right)$.

,,	21,	,,	$0\cdot9536 \times 10^{10}$,,
,,	22,	,,	$0\cdot9559 \times 10^{10}$,,
,,	23,	,,	$0\cdot9581 \times 10^{10}$,,
,,	24,	,,	$0\cdot9563 \times 10^{10}$,,
,,	26,	,,	$0\cdot9549 \times 10^{10}$,,

The mean value amounts to $0\cdot9557 \times 10^{10}\left(\dfrac{\text{millim.}}{\text{sec.}}\right)$.

<div align="center">

Series II.
P large, I_0 moderate.
Method 2.

</div>

Aug. 20, 1876, $0\cdot9516 \times 10^{10}\left(\dfrac{\text{millim.}}{\text{sec.}}\right)$.

,,	21,	,,	$0\cdot9545 \times 10^{10}$,,
,,	22,	,,	$0\cdot9550 \times 10^{10}$,,
,,	23,	,,	$0\cdot9575 \times 10^{10}$,,
,,	24,	,,	$0\cdot9556 \times 10^{10}$,,
,,	26,	,,	$0\cdot9552 \times 10^{10}$,,

The mean value is $0\cdot9549 \times 10^{10}$,,

<div align="center">

Series III.

P moderate, I_0 large.

Method 1.

Sept. 28, 1876, $0.9525 \times 10^{10} \left(\dfrac{\text{millim.}}{\text{sec.}}\right)$.
</div>

,,	29,	,,	0.9546×10^{10}	,,
,,	30,	,,	0.9581×10^{10}	,,
Oct.	1,	,,	0.9552×10^{10}	,,
,,	3,	,,	0.9557×10^{10}	,,
,,	4,	,,	0.9560×10^{10}	,,
The mean value is			0.9550×10^{10}	,,

<div align="center">

Series IV.

P moderate, I_0 large.

Method 2.

Sept. 28, 1876, $0.9568 \times 10^{10} \left(\dfrac{\text{millim.}}{\text{sec.}}\right)$.
</div>

,,	29,	,,	0.9561×10^{10}	,,
,,	30,	,,	0.9541×10^{10}	,,
Oct.	1,	,,	0.9552×10^{10}	,,
,,	3,	,,	0.9543×10^{10}	,,
,,	4,	,,	0.8589×10^{10}	,,
The mean value is			0.9559×10^{10}	,,

The final results of these measurements, effected under very diverse circumstances, agree within vanishingly small differences. They furnish the total result, that the absolute value of the Siemens empirical unit, derived from what takes place in voltaic induction called forth by sudden variations of current, amounts to $0.9554 \times 10^{10} \left(\dfrac{\text{millim.}}{\text{sec.}}\right)$.

On the basis of the laws of magneto-induction, according to the first method, we had found as the absolute value of Siemens's resistance-unit the quantity $0.9545 \times 10^{10} \left(\dfrac{\text{millim.}}{\text{sec.}}\right)$; this value accords to within $\frac{1}{1061}$ of its amount with that found by the second method. On account of the frequent repetition and manifold variation of the experiments, it may well be taken as sufficiently certain that this accordance is not accidental. From the almost perfect accordance of the final results obtained by the two methods two important conclusions can be drawn :—

(1) The fundamental laws hitherto recognized of induced currents of variable intensity represent with great precision

the real facts. The opinion of M. Lorenz, that the great difference between the results found by MM. W. Weber, F. Kohlrausch, and the physicists of the British Resistance Committee was the consequence of our imperfect knowledge of the laws of such induced currents, finds no confirmation at all in the above experiments.

(2) Absolute measurements of resistance can, with the means of galvanic observations nowadays at our disposal, be carried out with such exactness and certainty as can only be attained in few departments of physics. The notion widely accepted among physicists, that absolute measurements of resistance belong to those physical measurements which are capable of giving only roughly approximate values, and require peculiarly equipped localities for carrying them out—an opinion to which W. Siemens, among others, has given expression in the sentence, " we may certainly pronounce positively that even the most practised physicists, supplied with the most perfect instruments and localities, will not be able to make absolute resistance-measurements that would not differ by some percentage "—is refuted by the above results of experiment. According to my experience, these measurements can be effected with tolerable accuracy with very small means and in moderately equipped localities.

[To be continued.]

V. *Ice as an Electrolyte.*—*Second Communication.* By W. E. Ayrton *and* John Perry, *Professors in the Imperial College of Engineering, Tokio, Japan* [*].

[Plate II.]

IN our former paper on Ice as an Electrolyte, read before the Physical Society on May 26th of this year, we described experiments which proved, among other things, that as the temperature of ice is allowed to gradually rise the conductivity increases regularly, and that there is no sudden change in passing from the solid to the liquid state. We also determined roughly the specific inductive capacity of $\left\{ \begin{array}{c} \text{ice} \\ \text{water} \end{array} \right\}$ at $-13°{\cdot}5$ C. and at $+8°{\cdot}7$ C., and found that at the latter temperature it was about 2240 times as great as in the former. Preliminary experiments also showed us that there was very little change in the specific inductive capacity up to $0°$ C.; and it was anticipated that there would not be a very great change after $0°$; we therefore concluded that a very great change must

occur at the melting-point. A series of further experiments made with the same apparatus, since the writing of the previous aper, have enabled us to draw approximate curves (Pl. II.), A B C, D E for the specific capacity of $\left\{\begin{array}{c}\text{ice}\\\text{water}\end{array}\right\}$ from $-12°\cdot2$ C. to $+5°$ C., all tests of capacity being made by charging for ten seconds and then short-circuiting the $\left\{\begin{array}{c}\text{ice}\\\text{water}\end{array}\right\}$ condenser for fifteen seconds. From these it will be seen that, although the change at the melting-point is not quite as sudden as we expected, our anticipations are on the whole realized. It must be remembered too (see the description of the apparatus in our former paper) that, as the present experiments were made with a gradually rising temperature, the thermometer will always indicate a temperature a little higher than that of the ice ; the curve B C ought probably, therefore, to be even more vertical than it is.

Distances measured perpendicularly to Y O Y represent temperature—positive temperature if measured to the right, and negative if measured to the left. Distances measured perpendicularly to X O X represent specific capacity per cubic centimetre for points on the dotted lines, and conductivity for points on the continuous lines. The scale for temperature is the same for all the curves. The scale for vertical distances for the capacity-curve D E is one eighth of that for the capacity-curve A B C; and the scale for vertical distances for the conductivity-curve J K is three thousandths of that for the curve F G H.

The point A corresponds with a capacity per cubic centimetre of about $0\cdot002$ microfarad, at $-12°$ C. ; E corresponds with a capacity per cubic centimetre of about $0\cdot1185$ microfarad, at $+5°$ C. ; at this apparent temperature the capacity was increasing so rapidly as to make exact measurements very difficult, although the temperature was increasing but slowly. The point F corresponds with a specific resistance per cubic centimetre of about 2240 megohms, at $-12°\cdot4$ C. ; and K with a resistance per cubic centimetre of about $0\cdot34$ megohm, at $+11°\cdot02$ C. As in our previous experiments, the water employed in the water-condenser was distilled, and the ice was formed by freezing it with an external freezing-mixture no particle of which was allowed to fall into the distilled water.

The important theory which Prof. Clerk Maxwell has developed, by comparing the propagation of electro-magnetic disturbances through the ether with the propagation of light-vibrations, has been illustrated only by paraffin (a non-conductor);

and he has not considered the propagation of electro-magnetic disturbances in a conducting medium. But according to a former paper of ours, on the "Viscosity of Dielectrics," no dielectric can be assumed to be non-conducting, and the charging of any condenser whatever is always accompanied with absorption phenomena; also absorption certainly increases with conductivity.

Hence although, if a method of experimenting were employed in which a water-condenser of great internal resistance were discharged through wires of less and less resistance for shorter and shorter periods of time, the measured specific inductive capacity might get less and less, and gradually approach a value equal to the square of the index of refraction of water for infinitely long luminous waves (the index of refraction for air being called unity), still practically the measured specific inductive capacity can never be even approximately equal to the refractive index of water, since the absorbed charge is immeasurably greater than the surface-charge. We therefore need not expect to find the specific inductive capacity of water in its variations with temperature consistent with Dr. Gladstone's results for index of refraction. When Prof. Clerk Maxwell takes into account conductivity, his equations are not generally integrable; but even if they were they could not deal with the real case, because he leaves absorption quite out of account.

July 30, 1877.

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VI. Le Sage's *Theory of Gravitation.*
By JAMES CROLL, *LL.D., F.R.S.*[*]

LE SAGE'S theory of gravitation is at present exciting a good deal of attention among physicists. This is perhaps to a considerable extent owing to the fact that some of the conditions arbitrarily assumed by Le Sage in his hypothesis have been proved, from the kinetic theory of gases, to follow as necessary consequences.

A clear and able account of this theory has been given by Mr. Preston in the Philosophical Magazine for September and November last. Mr. Preston has endeavoured to answer all the objections which have been urged against the theory[†]. There is one objection, however, which appears to me not to

[*] Communicated by the Author.

[†] Mr. W. B. Taylor, in an interesting article on Kinetic Theories of Gravitation, published in the Smithsonian Report for 1876, lays down six fundamental characteristics of gravitation, with which every theory, he says, must agree. Of these six requirements, Le Sage's theory, he maintains, satisfies but two—namely (1) that the direction of gravity is radial

have been fully met. It is a necessary condition of Le Sage's theory, in order that gravity may be proportional to mass, that the total volume of the free spaces in a substance in the form of interstices between the molecules must be great compared with the total volume of matter contained in the molecules themselves. This condition of free interstices Mr. Preston considers to be satisfied by assuming the molecules to be small relative to their mean distances.

Were we at liberty to make any assumptions we choose in reference to the smallness of the molecules of matter and their distance apart, we might be able to satisfy the conditions of Le Sage's theory as to mass; but this we are not at liberty to do. Modern physics has enabled us to determine, at least roughly, the size of the ultimate molecules of matter and also their distance apart. This subject has recently been investigated by Sir William Thomson, the details of which will be found in a remarkable paper in 'Nature,' vol. i. p. 551. Sir William says the diameter of the molecule cannot be less than $\frac{1}{500,000,000}$ of a centimetre. The number of molecules in a cubic centimetre of a liquid or a solid may, he says, be from 3×10^{24} to 3×10^{26}. This gives the distance from centre to centre of two consecutive molecules to be from $\frac{1}{140,000,000}$ to $\frac{1}{460,000,000}$ of a centimetre. Now, if we take the mean of these two values, we have $\frac{1}{300,000,000}$ of a centimetre for the distance between the centres. The mean *spaces* between the molecules are therefore less than the diameter of the molecules themselves. Under this condition of things, it must be absolutely impossible that a gravific particle, even though it were *infinitely small*, could penetrate to the extent of a thousandth part of a centimetre into the interior of a body without having its motion stopped by coming into collision with a molecule. Le Sage's theory appears therefore to be utterly irreconcilable with Sir William's conclusions regarding the size of the material molecule. But even supposing we were to assume, what we are hardly warranted to do, that the molecules are 10,000 times smaller, and their distance apart 10,000 times greater than Sir William Thomson concludes, still this would not assist the theory. The gravific particles would then, no doubt, penetrate a little further into the interior of a body; but beyond a few feet, or perhaps a few inches, no particle could go.

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towards the acting mass, and (2) that its intensity diminishes as the square of the distance. But some of Mr. Taylor's objections have already been met by Mr. Preston in his memoir; beside, one or two of Mr. Taylor's fundamental postulates seem doubtful.

VII. *On the Artificial Production of Corundum, Ruby, and various Crystallized Silicates. By* MM. E. Fremy *and* Feil*.

SYNTHETIC mineralogy (that is to say, the artificial pro-
duction of minerals) presents, in a scientific point of view,
an interest which every one can understand ; for it throws the
greatest light upon the mode in which minerals are formed,
and permits us to solve certain questions, relative to their com-
position, which chemical analysis often leaves undecided. In
fact, a mineral which appears most pure contains almost always
foreign substances interposed which existed in the medium
that formed it ; analysis is then powerless to determine the
real composition of the mineral, while a synthetic reproduction
enables us to distinguish the constituent elements from those
which are merely accidental.

A great number of minerals have been artificially produced
in the dry way, in the wet way, and by M. Becquerel's inge-
nious methods ; and synthetic reproduction is daily receiving
some fresh extension, as is proved by the recent discoveries of
M. Hautefeuille.

Corundum has, perhaps, more than any other mineral ex-
ercised the sagacity of chemists. The excellent investigations
on the different modes of crystallization of alumina which have
been published by Ebelmen, de Senarmont, and since by
MM. H. Sainte-Claire Deville and Caron, by M. Gaudin, and
by M. Debray†, are known to every man of science. Even
after these remarkable researches, however, we have thought
we might still be permitted to interest the Academy by making
known the processes we employ for the production of differ-
ently coloured and crystallized alumina (that is to say, ruby
and sapphire) in masses sufficiently large to be used in horo-
logy and to be cut by the lapidary. It will probably be pos-
sible to apply the methods we are about to describe to the
artificial production of other minerals ; in this respect they
seem to possess a true scientific interest.

Wishing to approximate as nearly as possible to the natural
conditions which have probably determined the formation of
corundum, ruby, and sapphire, we have borrowed from industry
its most energetic heat-producing appliances, which permit an
elevated temperature to be produced, to be maintained for a
long time, and considerable masses to be operated on ; indeed

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† It is known that, by treating heated phosphate of alumina and lime
with chlorhydric acid, M. Debray has obtained at the same time apatite
and crystallized alumina.

we have often acted on 20 or 30 kilograms of material, which we kept heated uninterruptedly for twenty days.

It was in the oven of Feil's works that we conducted the experiments which required the highest temperature. When our trials demanded prolonged calcination, we had recourse to a glass-furnace which was generously placed at our disposal by the Company of Saint-Gobain. In this case our essays were directed by an eminent chemist, M. Henrivaux, whose intelligent supervision secured their success, for which we here render him all our thanks.

The following is the method which permitted us to produce the largest quantity of crystallized alumina :—

We commence by forming a fusible aluminate, and then heat it to bright redness with a siliceous substance. In this case the alumina is slowly separated from its saline combination in presence of a flux, and crystallizes.

We attribute the crystallization of the alumina to various causes :—either the volatilization of the base with which the alumina is united ; or the reduction of this base by the gases of the furnace ; or the formation of a fusible silicate which, by the combination of its silica with the base, isolates the alumina ; or, finally, a phenomenon of liquidation which produces a very fusible silicate and some hardly fusible alumina. All these cases presented themselves in our essays ; but the displacement of alumina by silica appears to us the surest process for effecting its crystallization.

Several fusible aluminates lend themselves to these different kinds of decomposition ; that which, up to the present, has given us the neatest results is the aluminate of lead. When a mixture of equal weights of alumina and minium is placed in a crucible of fire-clay, and calcined at a bright-red heat for a sufficient time, two different layers are found in the crucible after cooling : the one is vitreous, and formed chiefly of silicate of lead ; the other is crystalline, often presenting geodes filled with beautiful crystals of alumina. In this operation the sides of the crucible act by the silica which they contain. They are always made thinner, and often perforated, by the action of the lead-oxide ; therefore, to avoid loss of the product, we usually conduct the operation in a double crucible.

The experiment just described gives white crystals of corundum ; when we would obtain crystals presenting the rose-colour of the ruby, we add from 2 to 3 per cent. of bichromate of potass to the mixture of alumina and minium. The blue coloration of sapphire is produced by employing a small quantity of oxide of cobalt mixed with a trace of bichromate of potass. The ruby crystals thus obtained are ordinarily

coated with silicate of lead, which we remove in various ways— either by the action of fused oxide of lead, or by fluorhydric acid, or by potass in fusion, or by prolonged calcination in hydrogen, and afterwards by the action of alkalies and acids ; but in certain cases we find in the geodes some nearly pure crystals, which then exhibit all the characters of the natural corundums and rubies—possessing their composition, adamantine brilliance, hardness, specific gravity, and crystalline form.

Our rubies, in fact, scratch quartz and topaz ; their specific gravity is 4·0–4·1. They lose, like natural rubies, their rose-colour when strongly heated, and resume it on cooling. Submitted to lapidaries, they have been found as hard as, and often harder than, natural ones. They rapidly wear away the best grindstones of hardened steel. M. Jannettaz has kindly submitted our rubies to crystallographic observations ; with the Amici microscope they present a black cross in their interior and coloured rings upon the margins.

The crystals which we have had cut, and now exhibit to the Academy, have not yet the brilliance demanded by commerce, because they did not present to the lapidary faces favourable for cleavage and cutting ; but here are some crystalline masses weighing several kilograms, among which we shall doubtless find some that can be easily cut.

We will now describe the method which has enabled us to produce the fine specimens of crystallized silicates which we lay before the Academy. The experiments about to be described are connected with the preceding ; for they have frequently given us crystals of corundum together with crystallized silicates.

It was by means of fluorides that we produced the crystallized bodies, of which we have still to speak. In carrying out these researches we have had the opportunity of appreciating all the accuracy of the observations of M. Daubrée, who first demonstrated the important part played by fluorine, as a mineralizer, in the formation of mineral beds and of silicates. Those views are confirmed anew by our experiments.

Guided by the classic writings of M. Henri Sainte-Claire Deville, we have ascertained that, of all the mineralizers, perhaps the most active is the fluoride of aluminium. Submitting a mixture of equal weights of silica and fluoride of aluminium to a red heat during several hours, we verified that by the mutual reaction of the two substances fluoride of silicium is liberated, and a crystallized body is obtained which appears to be kyanite—that is, silicate of alumina. According to the determinations of M. Jannettaz, this body occurs in doubly refracting acicular crystals which extinguish light

obliquely with respect to their edges.   Doubtless they belong to one of the oblique systems—the oblique prism with rhombic base, or the doubly oblique prism.   These crystals gave the following composition :—

<div align="center">

Silica .... 47·65

Alumina .... 51·85

Loss........................... 0·50

</div>

This comes near to the composition of natural kyanite *.

The action of fluoride of aluminium on boric acid gave a crystallized borate of alumina which corresponds to kyanite. We are at present carrying out a series of trials in which fluoride of aluminium will act upon other mineral acids.

The important fact of the volatility of the fluoride of aluminium, discovered by M. Henri Sainte-Claire Deville, enables us readily to explain the remaining experiments.   When a mixture of equal weights of alumina and fluoride of barium, into which has been introduced 2 or 3 per cent. of bichromate of potass, is heated to and maintained at a very high temperature during a long time, a crystallized mass is obtained the study of which is of the greatest interest.   If the calcination has been effected in a crucible covered with another (which serves in some sort as a condenser), two sorts of crystals are found in the crucibles : the one sort are long colourless prisms, often several centims. in length, and presenting the aspect of the silvery flowers of antimony ; the others are ruby crystals, remarkable for the regularity of their forms and beautiful rose-colour.

The long prismatic colourless crystals are formed by a double silicate of baryta and alumina, which present the composition :—

<div align="center">

Silica .... 34·32

Baryta........................ 35·04

Alumina .... 30·37

</div>

In our essays this double silicate often crystallized in rather short, hard and transparent, clinorhombic prisms which, M. Terreil has ascertained, have the same composition as the long and hollow prismatic needles.

M. Jannettaz has proved that the long prisms are often constituted by four plates with parallel faces, forming the faces of a hollow prism.   These plates are very thin ; under the microscope they extinguish light ; or rather they let darkness persist between two Nicols, parallel to their mutual intersections ; the

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* The crystals we obtained are very easily produced, but are not of large size ; they may therefore belong to those fibrous varieties of dysthene which have been described under the names of Fribolite, Bucholzite, Bamlite, and Sillimanite.

plane of the optic axes is parallel to these intersections ; they cut one another at angles of $60° 42'$ and $119° 18'$.

There is therefore produced in this curious reaction corundum and a crystallized double silicate. These two crystalline substances result from the following transformations :—

In the calcination of the mixture of alumina and fluoride of barium there are evidently formed fluoride of aluminium and baryta. The fluoride of aluminium, once produced, must have acted in two different ways. Decomposed by the gases from the hearth, it formed fluorhydric acid and corundum, which crystallized under the influence of the vapours. Acting besides upon the silica of the crucible, it gave rise to silicate of alumina, which, combining with the baryta, produced the fine crystals of double silicate of alumina and baryta which we exhibit to the Academy. Such, in our opinion, is the theory of the reaction.

Permit us now to dwell on the conditions which have determined the crystallization of the two substances, corundum and the double silicate. Looking at the specimens we here exhibit, and which present such well-defined crystals, one is struck with the place which they occupy in the crucibles : they seem to have been volatilized ; and yet we have ascertained, by exposing them to the highest temperatures of our furnaces, that they are absolutely fixed. It is because the fluorides are not merely powerful mineralizers ; they are also compounds which, as was formerly said, give wings to the least-volatile substances. Do we not recollect, indeed, that remarkable formation of orthose felspar, produced artificially and found in the upper part of a copper-furnace at Mansfeld ? The employment of fluoride of calcium in the melting-bed of the furnace which produced that felspar permits the belief that the fluorine intervened in that case as a transporting agent. It was evidently this which presented itself in our experiments, as in those which have been so often performed by M. H. Sainte-Claire Deville : the agents of the transport and crystallization of the corundum and the silicate were likewise the fluorine compounds which we employed.

It was to be presumed that this action of fluoride of barium upon alumina in presence of silica, forming a crystallized double silicate, would reappear as a general phenomenon connected with the decomposition of the fluorides by different bases. This we have, in fact, proved ; in another communication we will describe some crystallized double silicates produced under the same conditions as the double silicate of alumina and baryta ; and then we shall give the general formulæ of these compounds.

Such is the brief account of our researches which we wished to present to-day to the Academy. It is probable that our experiments, which give, in considerable masses, substances whose hardness is comparable to that of the natural ruby, will be utilized from time to time by the watchmaker, and even by the jeweller. We will say, in conclusion, that in this labour the aim we pursue is exclusively scientific; consequently we put into the possession of the public the facts we have discovered, and shall be very happy to learn that they have found useful industrial applications.

VIII. *On a Variety of the Mineral* Cronstedite. *By* Frederick Field, *F.R.S., Vice-President of the Chemical Society**.

THE various analyses of the interesting mineral *Cronstedite*, named after the Swedish mineralogist Cronstedt and hitherto found only in two localities (Przibram in Bohemia and Wheal Maudlin in Cornwall), are rather conflicting, since the amounts of ferrous and ferric oxide differ considerably, as the following results will show. Nevertheless all examinations tend to prove that Cronstedite is essentially a hydrous silicate of ferrous and ferric oxide.

From four specimens from Przibram we have:—

Silicic acid	22·452
Ferric oxide	——
Ferrous oxide	58·852
Manganous oxide.........	5·078
Magnesium oxide	2·885
Water	10·700
	99·967

This was corrected by Von Kobell, after a determination of the degree of the oxidation of the iron, which gave:—

Silicic acid	22·452
Ferric oxide	35·350
Ferrous oxide	27·112
Manganous oxide	5·078
Mangnesium oxide	2·885
Water	10·700
	103·577

And in two more analyses, one by Steinmann and one by Damour, the ferrous oxide varied more than 2 per cent.:—

	SiO_2.	Fe_2O_3.	FeO.	MnO.	MgO.	H_2O.
Steinmann...	22·83	29·08	31·44	3·43	3·25	10·70
Damour......	21·39	29·08	33·52	1·01	4·02	9·76

Messrs. Maskelyne and Flight, in a valuable paper upon certain Cornish and other minerals (*vide* 'Journal of the Chemical Society,' new series, vol. ix. p. 9), gave the results of some analyses of specimens of Cronstedite from Cornwall, handed them by Mr. Talling; but these, again, are not very concordant.

The first analysis of this mineral gave the following numbers:—

Iron protoxide .... 36·307
Iron peroxide .... 36·762
Silicic acid .... 17·468
Water .... 10·087
Calcium oxide .... ·087
　　　　　　　　　　　　　────
　　　　　　　　　　　　　100·711

A second analysis, with a fresh and more carefully selected material, gave the following percentages :—

Iron protoxide .... 38·570
Iron peroxide .... 32·752
Silicic acid .... 18·546
Water .... 10·132
　　　　　　　　　　　　　────
　　　　　　　　　　　　　100·000

Mr. Talling called my attention to an amorphous, dark leek-green mineral, at times associated with Cronstedite, which struck me as interesting, inasmuch as, although differing so widely at first sight from the brilliant black of the latter, yet had exactly the appearance and colour of the *streak* of Cronstedite after abrasion with a file or some hard mineral.

Qualitative examination proved it to consist entirely of ferric and ferrous oxides, silicic acid, and water. Its specific gravity was 3, hardness about 2·5. On heating, water was evolved, and the green powder rapidly passed into yellowish brown. No traces of either magnesium- or manganic oxide could be detected (as in the case of the Bohemian mineral); and there was no evolution of carbonic acid on the addition of weak hydrochloric acid, by which it is instantly decomposed with separation of silica and solution of the two iron oxides.

A quantitative analysis yielded:—

Ferrous oxide .... 39·46
Ferric oxide .... 18·51
Silicic acid .... 31·72
Water........................ 11·02
　　　　　　　　　　　　　────
　　　　　　　　　　　　　100·71

It would be useless perhaps to attempt to give a formula to the above ; and the entire absence of crystallization deprives it of much of the interest it should otherwise possess.

The following, perhaps, would give the best idea of its constitution :—

$$3\,(\text{FeO, SiO}_2\,\text{H}_2\,\text{O}) + \text{Fe}_2\,\text{O}_3.$$

	Found.	Calculated.
Ferrous oxide	39·46	40·74
Ferric oxide	18·51	15·09
Silicic acid............	31·72	33·96
Water	11·02	10·18
	100·71	99·97

It may be merely a coincidence ; but it is worthy of remark that the water in the mineral just described and that in the Cronstedite examined by Messrs. Maskelyne and Flight[*] is much about the same, which can also be said of the ferrous oxide, neither of them varying 1 per cent., while the silicic acid and ferric oxide seem, so to speak, to have changed places.

	Cronstedite.	Green mineral.
Silicic acid	18·546	31·72
Ferric oxide	32·752	18·51
	51·298	50·23

It has already been remarked that, on heating the green mineral, its colour is changed to yellowish brown; and on examination of the residue, no trace of ferrous oxide could be detected. When the water has been drawn off, at the lowest possible temperature, and the mineral further heated, it rapidly gains in weight from absorption of oxygen.

IX. *On the Distribution of Electricity on two Spherical Surfaces.* By Prof. Cayley[†].

IN the two memoirs " Sur la Distribution de l'Electricité à la Surface des Corps Conducteurs," *Mém. de l'Inst.* 1811, Poisson considers the question of the distribution of electricity upon two spheres : viz. if the radii be a, b, and the distance of the centres be c (where $c > a + b$, the spheres being exterior to each other), and the potentials within the two spheres respectively have the constant values h and g, then—

for Poisson's $f\left(\dfrac{x}{a}\right)$ writing $\phi(x)$, and for his $\text{F}\left(\dfrac{x}{b}\right)$ writing

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[*] From the most carefully selected specimen.
[†] Communicated by the Author.

$\Phi(x)$—the question depends on the solution of the functional equations

$$a\phi(x) + \frac{b^2}{c-x} \Phi\left(\frac{b^2}{c-x}\right) = h,$$

$$\frac{a^2}{c-x} \phi\left(\frac{a^2}{c-x}\right) + b\Phi(x) = g,$$

where of course the x of either equation may be replaced by a different variable.

It is proper to consider the meaning of these equations : for a point on the axis, at the distance x from the centre of the first sphere, or say from the point A, the potential of the electricity on this spherical surface is $a\phi x$ or $\frac{a^2}{x}\phi\left(\frac{a^2}{x}\right)$, according as the point is interior or exterior ; and, similarly, if x now denote the distance from the centre of the second sphere (or, say, from the point B), then the potential of the electricity on this spherical surface is $b\Phi x$ or $\frac{b^2}{x}\Phi\left(\frac{b^2}{x}\right)$, according as the point is interior or exterior ; $\phi(x)$ is thus the same function of (x, a, b) that $\Phi(x)$ is of (x, b, a). Hence, first, for a point interior to the sphere A, if x denote the distance from A, and therefore $c-x$ the distance of the same point from B, the potential of the point in question is

$$= a\phi x + \frac{b^2}{c-x} \Phi\left(\frac{b^2}{c-x}\right);$$

and, secondly, for a point interior to the sphere B, if x denote the distance from B and therefore $c-x$ the distance of the same point from A, the potential of the point is

$$= \frac{a^2}{c-x}\phi\left(\frac{a^2}{c-x}\right) + b\Phi(x);$$

The two equations thus express that the potentials of a point interior to A and of a point interior to B are $= h$ and g respectively.

It is to be added that the potential of an exterior point, distances from the points A and B $= x$ and $c-x$ respectively, is

$$= \frac{a^2}{x}\phi\left(\frac{a^2}{x}\right) + \frac{b^2}{c-x} \Phi\left(\frac{b^2}{c-x}\right);$$

and that by the known properties of Legendre's coefficients, when the potential upon an axial point is given, it is possible to pass at once to the expression for the potential of a point not on the axis, and also to the expression for the electrical density at a point on the two spherical surfaces respectively.

The determination of the functions $\phi(x)$ and $\Phi(x)$ gives thus the complete solution of the question.

I obtain Poisson's solution by a different process as follows:—Consider the two functions

$$\frac{a^2(c-x)}{c^2-b^2-cx}, \quad = \frac{\mathrm{a}x+\mathrm{b}}{\mathrm{c}x+\mathrm{d}} \text{ suppose,}$$

and

$$\frac{b^2(c-x)}{c^2-a^2-cx}, \quad = \frac{\alpha x+\beta}{\gamma x+\delta} \text{ suppose;}$$

and let the nth functions be

$$\frac{\mathrm{a}_n x+\mathrm{b}_n}{\mathrm{c}_n x+\mathrm{d}_n} \text{ and } \frac{\alpha_n x+\beta_n}{\gamma_n x+\delta_n}$$

respectively.

Observing that the values of the coefficients are

$$\begin{pmatrix} \mathrm{a}, & \mathrm{b} \\ \mathrm{c}, & \mathrm{d} \end{pmatrix} = \begin{pmatrix} -a^2, & a^2c \\ -c, & c^2-b^2 \end{pmatrix}, \text{ and } \begin{pmatrix} \alpha, & \beta \\ \gamma, & \delta \end{pmatrix} = \begin{pmatrix} -b^2, & b^2c \\ -c, & c^2-a^2 \end{pmatrix},$$

so that we have

$$\mathrm{a}+\mathrm{d}=\alpha+\delta, \ = c^2-a^2-b^2, \ \mathrm{ad}-\mathrm{bc}=\alpha\delta-\beta\gamma, \ = a^2b^2,$$

and consequently that the two equations

$$\frac{(\lambda+1)^2}{\lambda}=\frac{(\mathrm{a}+\mathrm{d})^2}{\mathrm{ad}-\mathrm{bc}}, \quad \frac{(\lambda+1)^2}{\lambda}=\frac{(\alpha+\delta)^2}{\alpha\delta-\beta\gamma}$$

are in fact one and the same equation

$$\frac{(\lambda+1)^2}{\lambda}=\frac{(c^2-a^2-b^2)^2}{a^2b^2}$$

for the determination of λ, then (by a theorem which I have recently obtained) we have the following equations for the coefficients

$$\begin{pmatrix} \mathrm{a}_n, & \mathrm{b}_n \\ \mathrm{c}_n, & \mathrm{d}_n \end{pmatrix}, \quad \begin{pmatrix} \alpha_n, & \beta_n \\ \gamma_n, & \delta_n \end{pmatrix}$$

of the nth functions; viz. these are :—

$$\mathrm{a}_n x+\mathrm{b}_n = \frac{1}{\lambda^2-1}\left(\frac{\mathrm{a}+\mathrm{d}}{\lambda+1}\right)^{n-1}\{(\lambda^{n+1}-1)(\mathrm{a}x+\mathrm{b})+(\lambda^n-\lambda)(-\mathrm{d}x+\mathrm{b})\},$$

$$\mathrm{c}_n x+\mathrm{d}_n = \quad ,, \quad ,, \quad \{(\lambda^{n+1}-1)(\mathrm{c}x+\mathrm{d})+(\lambda^n-\lambda)(\quad \mathrm{c}x-\mathrm{a})\};$$

and similarly

$$\alpha_n x+\beta_n = \frac{1}{\lambda^2-1}\left(\frac{\alpha+\delta}{\lambda+1}\right)^{n-1}\{(\lambda^{n+1}-1)(\alpha x+\beta)+(\lambda^n-\lambda)(-\delta x+\beta)\}$$

$$\gamma_n x+\delta_n = \quad ,, \quad ,, \quad \{(\lambda^{n+1}-1)(\gamma x+\delta)+(\lambda^n-\lambda)(\quad \gamma x-\alpha)\}.$$

Observe that these equations give, as they ought to do,

$a_0 x + b_0 = x$, $c_0 x + d_0 = 1$, $a_1 x + b_1 = ax + b$, $c_1 x + d_1 = cx + d$;

and similarly

$\alpha_0 x + \beta_0 = x$, $\gamma_0 x + \delta_0 = 1$, $\alpha_1 x + \beta_1 = \alpha x + \beta$, $\gamma_1 x + \delta_1 = \gamma x + \delta$.

Substituting in the first two equations $\dfrac{a^2}{c-x}$ in place of x, and in the second two equations $\dfrac{b^2}{c-x}$ in place of x, we obtain the following results which will be useful :—

$$a_n a^2 + b_n(c-x) = a^2(\gamma_n x + \delta_n),$$
$$c_n a^2 + d_n(c-x) = \frac{1}{b^2}(\alpha_{n+1} x + \beta_{n+1}),$$
$$\alpha_n b^2 + \beta_n(c-x) = b^2(c_n x + d_n),$$
$$\gamma b^2 + \delta_n(c-x) = \frac{1}{a^2}(a_{n+1} x + b_{n+1}),$$

the last two of which are obtained from the first two by a mere interchange of letters ; it will therefore be sufficient to prove the first and second equations.

For the first equation we have

$$a_n a^2 + b_n(c-x) = \frac{1}{\lambda^2-1}\left(\frac{a+d}{\lambda+1}\right)^{n-1}\{(\lambda^{n+1}-1)[aa^2 + b(c-x)]$$
$$+ (\lambda^n - \lambda)[-da^2 + b(c-x)]\},$$

where the term in { } is =

$(\lambda^{n+1}-1)[-a^4 + a^2 c(c-x)] + (\lambda^n-\lambda)[a^2(b^2-c^2) + a^2 c(c-x)]$;

viz. this is

$$= a^2\{(\lambda^{n+1}-1)(c^2-a^2-cx) + (\lambda^n-\lambda)(b^2-cx)\} ;$$

or it is

$$= a^2\{(\lambda^{n+1}-1)(\gamma x + \delta) \quad + (\lambda^n-\lambda)(\gamma x-\alpha)\},$$

whence the relation in question.

The proof of the second equation is a little more complicated : we have

$$c_n a^2 + d_n(c-x) = \frac{1}{\lambda^2-1}\left(\frac{a+d}{\lambda+1}\right)^{n-1}\{(\lambda^{n+1}-1)[ca^2 + d(c-x)]$$
$$+ (\lambda^n-\lambda)[ca^2 - a(c-x)]\}$$

where the term in { } is =

$(\lambda^{n+1}-1)[-ca^2 + (c^2-b^2)(c-x)] + (\lambda^n-\lambda)[-ca^2 + a^2(c-a)]$.

Comparing this with

$$\alpha_{n+1}x + \beta_{n+1} = \frac{1}{\lambda^2+1}\left(\frac{\alpha+\delta}{\lambda+1}\right)^n\{\lambda^{n+2}-1)(\alpha x+\beta) + (\lambda^{n+1}-\lambda)(-\delta x+\beta)\},$$

where the term in $\{\ \ \}$ is

$$=(\lambda^{n+2}-1)[b^2(c-x)] + (\lambda^{n+1}-\lambda)[-c(c^2-a^2-b^2)+(c^2-a^2)(c-x)],$$

it is to be observed that the quotient of the two terms in $\{\ \ \}$ is in fact a constant; this is most easily verified as follows. Dividing the first of them by the second we have a quotient which when $x=c$ is

$$\frac{(\lambda^{n+1}-1)(-ca^2)+(\lambda^n-\lambda)(-ca^2)}{(\lambda^{n+1}-\lambda)(-c(c^2-a^2-b^2))}, = \frac{a^2(\lambda^{n+1}-1+\lambda^n-\lambda)}{(\lambda^{n+1}-\lambda)(c^2-a^2-b^2)},$$

$$= \frac{a^2(\lambda+1)}{(c^2-a^2-b^2)\lambda};$$

and when $x=0$ is

$$\frac{(\lambda^{n+1}-1)c(c^2-a^2-b^2)}{(\lambda^{n+2}-1)b^2c+(\lambda^{n+1}-\lambda)b^2c}, = \frac{(\lambda^{n+1}-1)(c^2-a^2-b^2)}{(\lambda^{n+2}-1+\lambda^{n+1}-\lambda)b^2}$$

$$\frac{c^2-a^2-b^2}{b^2(\lambda+1)}:$$

these two values are equal by virtue of the equation which defines λ; and hence the quotient of the two linear functions having equal values for $x=c$ and $x=0$, has always the same value; say it is $=\frac{c^2-a^2-b^2}{b^2(\lambda+1)}$. Hence, observing that $a+d = \alpha+\delta, =c^2-a^2-b^2$, the quotient, $c_n a^2 + d_n(c-x)$ divided by $\alpha_{n+1}x+\beta_{n+1}$, is

$$= \frac{\lambda+1}{c^2-a^2-b^2} \cdot \frac{c^2-a^2-b^2}{b^2(\lambda+1)}, = \frac{1}{b^2};$$

or we have the required equation

$$c_n a^2 + d_n(c-x) = \frac{1}{b^2} \cdot (\alpha_{n+1}x+\beta_{n+1}).$$

Considering now the functional equations, suppose for the moment that g is $=0$; the two equations may be satisfied by assuming

$$\phi(x) = h\left\{\frac{1}{c_0 x+d_0} + \frac{\omega}{c_1 x+d_1} + \dots\right\}L,$$

$$\Phi(x) = -h\left\{\frac{\omega}{\alpha_1 x+\beta_1} + \frac{\omega^2}{\alpha_2 x+\beta_2} + \dots\right\}M,$$

We in fact, from the foregoing relations, at once obtain

$$\frac{a^2}{c-x} \phi \frac{a^2}{c-x} = h \left\{ \frac{\omega}{\alpha_1 x + \beta_1} + \frac{\omega^2}{\alpha_2 x + \beta_2} \cdots \right\} \frac{a^2 b^2 L}{\omega},$$

$$\frac{b^2}{c-x} \Phi \frac{b^2}{c-x} = -h \left\{ \frac{\omega}{c_1 x + d_1} + \frac{\omega^2}{c_2 x + d_2} \cdots \right\} M.$$

To satisfy the first equation we must have $M = aL$; viz., this being so, the equation becomes

$$a\phi x + \frac{b^2}{c-x} \Phi\left(\frac{b^2}{c-x}\right) = \frac{aLh}{c_0 x + d_0};$$

or, since $c_0 x + d_0 = 1$, the equation will be satisfied if only $aL = 1$, whence also $M = 1$. And the second equation will be satisfied if only $\dfrac{a^2 b^2 L}{\omega} = bM$; viz., substituting for L, M their value, we find $\omega = ab$.

Supposing, in like manner, that $h = 0$, g retaining its proper value, we find a like solution for the two equations; and by simply adding the solutions thus obtained, we have a solution of the original two equations

$$a\phi(x) + \frac{b^2}{c-x} \Phi\left(\frac{b^2}{c-x}\right) = h,$$

$$\frac{a^2}{c-x} \phi\left(\frac{a^2}{c-x}\right) + b\Phi(x) = g;$$

viz. the solution is

$$\phi(x) = \frac{h}{a} \left\{ \frac{1}{c_0 x + d_0} + \frac{ab}{c_1 x + d_1} + \cdots \right\} - g \left\{ \frac{ab}{a_1 x + b_1} + \frac{(ab)^2}{a_2 x + b_2} + \cdots \right\}$$

$$\Phi(x) = -h \left\{ \frac{ab}{\alpha_1 x + \beta_1} + \frac{(ab)^2}{\alpha_2 x + \beta_2} + \cdots \right\} + \frac{g}{b} \left\{ \frac{1}{\gamma_0 x + \delta_0} + \frac{ab}{\gamma_1 x + \delta_1} + \cdots \right\}$$

We have a general solution containing an arbitrary constant P by adding to the foregoing values

for ϕx a term $= \dfrac{Pb(a-b)}{\sqrt{a^2(c-x) - x(c^2 - b^2 - cx)}},$

and for Φx a term

$$= \frac{Pa(b-a)}{\sqrt{b^2(c-x) - x(c^2 - a^2 - cx)}},$$

as may be easily verified if we observe that the function

$$a^2(c-x) - x(c^2 - b^2 - cx),$$

writing therein $\dfrac{a^2}{c-x}$ for x, becomes

$$= \dfrac{a^2}{(c-x)^2}\{b^2(c-x)-x(c^2-a^2-cx)\},$$

and similarly that

$$b^2(c-x)-x(c^2-a^2-cx),$$

writing therein $\dfrac{b^2}{c-x}$ for x, becomes

$$= \dfrac{b^2}{(c-x)^2}\{a^2(c-x)-x(c^2-b^2-cx)\}.$$

More generally, the terms to be added are for ϕx a term as above, where P denotes a function of x which remains unaltered where x is changed into $\dfrac{a^2(c-x)}{c^2-b^2-cx}$, and for Φx a term as above with P′ instead of P, where P′ denotes what P becomes when x is changed into $\dfrac{a^2}{c-x}$. But these additional terms vanish for the electrical problem, and the correct values of ϕx, Φx are the particular values given above.

It is to be remarked that the function

$$\dfrac{a^2(c-x)}{c^2-b^2-cx} \text{ is } = \dfrac{a^2}{c-\dfrac{b^2}{c-x}};$$

viz., considering x as the distance of a point X from A, then taking the image of X in regard to the sphere B, and again the image of this image in regard to the sphere A, the function in question is the distance of this second image from A. And similarly the function

$$\dfrac{b^2(c-x)}{c^2-a^2-cx} \text{ is } = \dfrac{b^2}{c-\dfrac{a^2}{c-x}};$$

viz., considering here x as the distance of the point X from B, then taking the image of X in regard to the sphere A, and again the image of this image in regard to the sphere B, the function in question is the distance of this second image from B. It thus appears that Poisson's solution depends upon the successive images of X in regard to the spheres B and A alternately, and also on the successive images of X in regard to the spheres A and B alternately. This method of images is in fact employed in Sir W. Thomson's paper " On the Mutual Attraction or Repulsion between two Electrified Spherical Conductors," Phil. Mag., April and August 1853.

X. On the Destruction of the undeveloped Photographic Image. By Captain ABNEY, R.E., F.R.S.*

IT has always seemed that more experiments were required in regard to the destruction of the undeveloped photographic image by chemical or physical agency. In the present communication I propose to give some instances of the destruction by the former, as it appears they may be capable of throwing light on some of the phenomena which have as yet been only imperfectly explained.

The undeveloped Daguerrean image, as is well known, can be destroyed by the action of iodine, bromine, or chlorine on the sensitized surface of the plate ; and it can also be destroyed by other agents which might naturally be expected to do so. Perhaps the most remarkable method of destroying the image, however, is by the action of the rays lying at the least-refrangible end of the spectrum. Draper and others have applied this to obtain an image of this portion of the solar spectrum by submitting a plate to which had been given a preliminary exposure to its action. On development with mercury, a negative picture of the red end of the spectrum was obtained, together with a positive picture of the violet end.

With iodide of silver formed in a film of collodion, the image is known to be destroyed by potassium iodide, probably because it forms a definite compound with the silver image. Sulphuretted hydrogen, coal-gas, and other similar bodies also destroyed the image, by causing a reduction of silver all over the film. This last phenomenon scarcely need be considered here, as it is chiefly dependent on the silver nitrate which kept on the film for sensitizing-purposes.

Very few, if any, experiments are recorded on the destruction of the image on silver bromide, principally, it may be presumed, because the use of that sensitive salt of silver has only become general within the last few years. The practical photographer well knows the great difficulties that are met with in sensitizing an entirely bromized collodion in the silver-nitrate bath ; and until the emulsion process was introduced in a practical form, experimenting with silver bromide to any great extent was an unsatisfactory undertaking.

In what is known as the " washed collodio-bromide emulsion process " we are now able to prepare films containing silver bromide in which neither soluble bromide nor yet silver nitrate are in excess, and experimenting is more easily performed even than with silver iodide.

There has always, however, been one drawback to this pro-

* Communicated by the Author.

cess; and that is, the danger of making emulsified silver bromide which should give a veiled image. Various means of overcoming this difficulty have from time to time been employed; and it was in my endeavour to discover the reason why they were effective that I was led to make other experiments.

Now the veil (or fog, as it is more commonly designated) seemed to depend on similar chemical changes in the bromide film to those on which the existence of the image itself depended. By finding out the cause of one, it seemed probable that the reason of the other might be discovered.

It was found, if films made by the silver-bromide emulsion, which on development gave an unveiled image, were exposed to light and then treated with (1) bromine, (2) iodine, (3) nitric acid, (4) sulphuric acid, (5) cupric bromide, (6) ferric chloride, and other similar bromides and chlorides which easily part with a portion of their bromine or chlorine to metallic silver, that on development no trace of the action of light need be apparent. If we suppose that the photographic image is dependent on the reduction of the argentic to a state of argentous bromide, the action of (1), (2), (5), and (6) would be easily accounted for: (1) and (2) would furnish one of the atoms (which, for convenience sake, I will call the *loose atom*) of silver with the necessary atoms to saturate it. Thus:—

$$2\,Ag_2\,Br + 2\,I = Ag_2\,Br_2 + Ag_2\,I_2.$$
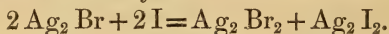
Similarly, by a decomposition of cupric bromide or the ferric chloride, the same reaction would be obtained:—

$$2\,Ag_2\,Br + 2\,Cu\,Br_2 = 2\,Ag_2\,Br_2 + 2\,CuBr.$$
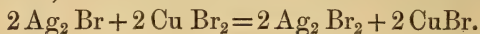
The action of nitric acid and sulphuric acid was more difficult of explanation, unless it were believed that one atom of the argentous bromide was completely removed. When testing silver-iodide films with these bodies, it was found that nitric acid did not destroy the image, but that it could be developed after its application. This experiment seems to prove that the image on the iodide was not caused by a separation of metallic silver. The question then arose as to whether these images were formed by argentous iodide at all, in which case it might be considered that the chemical theory failed. In order to obtain more evidence, it was thought advisable to try whether the loose atom of silver, if it existed, could be oxidized. If this could be effected, it seemed probable that the image would be undevelopable.

Silver bromide was first put to the test. Plates were treated with potassium bichromate, potassium permanganate, or with chromic acid. In every case the image was obliterated and the film was in a state to receive another developable impression.

The image on silver iodide was amenable to the last two oxidizing agents, but apparently not to the first one.

A silver-bromide plate, after the image had been oxidized, was allowed to come into contact with nascent hydrogen ; and a partial restoration of the image was obtained, but the distinctness was much marred by the reducing action that took place on the silver bromide which had received no impression by light.

As a crucial test, however, both bromide and iodide films were exposed moistened to the action of ozone obtained by the ordinary electrical arrangements from perfectly pure oxygen ; and in all cases the image was totally destroyed. Whilst the film was still in the ozonized condition, it was again exposed, and a feeble image, due to the new exposure, was developable, whilst after destroying the ozone, a new exposure gave a vigorous picture.

From these experiments are to be deduced that the image formed in the silver iodide is of the same nature as that formed in the silver bromide, the difference between them being probably that in the former the atoms are more strongly bonded than in the latter, that oxidation of the loose atom of silver makes the image undevelopable, and that to this cause ·the deterioration of the image on dry plates by keeping after exposure is most probably due. If the red rays promote oxidation, as has recently been asserted by Chastaing, the phenomena observed by Draper and others, already alluded to, may be readily accounted for.

XI. *Notices respecting New Books.*

Report on the Administration of the Meteorological Department of India in 1875–76. Government Central Press.

Report on the Meteorology of India in 1875. *By* HENRY F. BLANFORD, *Meteorological Reporter to the Government of India.* First Year. Calcutta, 1877.

THE first of the above-named publications deals principally with the official establishment by the Government of India of the Meteorological Department on the 27th of September, 1875, sanctioning a scheme of reorganization recommended in the author's Report of the 26th of July, 1875. To this report the author's name is not appended ; but, from a remark on the publications of the Department, we believe it to be the production of Henry F. Blanford, the Meteorological Reporter to the Government of India. To this report of the administration of the Department are appended some extracts from a report by Mr. F. Chambers, which contain some interesting notices. It appears from these extracts that the first attempt at systematic Meteorological registration at Provincial

stations in the Bombay Presidency was made in 1851—two years before the Brussels Conference, the outcome of which, among other results, was the establishment of our own Meteorological Office under the direction of the late Admiral FitzRoy, and about four years after the general idea of systematic meteorological observation, especially by officers in the army, navy, and mercantile marine, was suggested in this country. The observations in the Bombay Presidency were made in pursuance of orders received from the Honourable Court of Directors; but at most of the stations they were soon discontinued altogether, whilst at others they were continued in a very inefficient and negligent manner. Towards the end of 1852 the receipt of five complete sets of verified instruments from England revived in a measure the work of observation in India. Orders were given to erect them at Belgaum, Poona, Bombay, Deesa, and Kurrachee, and place them in charge of the Superintendent of the Government Observatory at Bombay, and the senior Medical Officers at the other stations, with this comment :—" We would hope that, from the zeal and energy of Medical Officers in charge of European hospitals and their love of science, the observations may be made by themselves and their establishments, without entailing on the public any expense on this account." The zeal and energy of the medical officers and their love of science, however, seem not to have been equal to the occasion; for after vainly endeavouring until the end of 1855 to carry out the orders they had received without entailing expense on the public, it was arranged at the direction of the Honourable Board that two European soldiers should be told off at each station to undertake the duty of making meteorological observations, on an allowance of 25 rupees per month for each observatory. The soldiers were sent to the Bombay Observatory early in 1856 for a preparatory course of training, on the successful completion of which they were furnished with certificates of competency for performing the work. Soon after this time the real work of meteorological registration may be said to have commenced so far as the observers were concerned; for the work from this time appears to have been carried on generally in a thorough and trustworthy manner.

Turning to the administrative report, the first portion of which has reference to the machinery of the Meteorological Departments previous to the establishment of the Meteorological Department by the Government and the reorganization of the observations, we find there were eighty-four observatories in India and its Dependencies. In the scheme of reorganization, one of the most important points was a redistribution of the observatories, in effecting which it was proposed to group low-lying and elevated observatories in pairs, in order to throw light on the variations of the atmosphere in a vertical direction. The result of the reorganization has been the establishment of 95 observatories, viz. 3 first class, 21 second class, 71 third class, which, with 9 independent observatories furnishing data, make a total of 104 meteorological observatories actively at work in India.

Under the head of Publications we find the results obtained by the Meteorological Department of India will be published in two serial forms—an 'Annual Report on the Meteorology of India,' and 'Indian Meteorological Memoirs,' the part of which about to be issued will contain :—

1st. On the Winds of Calcutta.

2nd. On the Climate and Meteorology of Káshghar and Yárkánd.

3rd. On the Diurnal Variation of the Barometric Pressure at Simla.

The Report on the Meteorology of India is a folio of 386 pages, 297 being occupied with tables of results obtained at 87 stations. These comprise Solar Radiation, Terrestrial Radiation, Air-temperature, Atmospheric Pressure, Anemometry, Hygrometry, Cloud-proportion, and Rainfall. The second paragraph of the introductory portion of this Report indicates in so lucid a manner the connexion of Meteorology with Physical Geography, and is of itself sufficient to exhibit the spirit in which Mr. Blanford has undertaken the work, that we quote it *in extenso* :—

" As a field for the advantageous study of Physical Meteorology, India stands pre-eminent—in virtue not only of the intensity and variety of the phenomena it presents, but also of their intimate localization in a circumscribed arena. Isolated on the north by the gigantic mountain-range, which presents an impassable obstacle to any interchange of the lower half of the atmosphere with that of the regions beyond, and bathed on two sides by an ocean which stretches away without a break to the margin of the Antarctic land, it affords an almost unique example of the contrasted conditions of land and water, of continent and ocean, of great extent, yet for the most part accessible and uncomplicated by influences of unknown origin and uncertain magnitude. At the same time situated half within and half without the tropics, its southern extremity traversed by the terrestrial equator of heat, and dominated during five months of the year by a vertical sun, it receives in its greatest intensity the solar heat which is the source of all meteorological action ; and yet again so vast is its extent, and so varied are the physico-geographical characteristics of its different parts, that it exhibits within itself at one and the same moment extreme examples of the most opposite effects of that energy in the parching heat of the Scindian deserts and the torrential rains of the Gháts and of Eastern Bengal."

It is thus with a view of unfolding the laws of the physical interdependence of Meteorological phenomena with the geographical features of a country, that Mr. Blanford has undertaken and effectively carried out the duty intrusted to him. A considerable portion of the introductory matter of this first annual volume consists of a most interesting and valuable sketch of the Physical Geography of India and its Dependencies, followed by a description of the physical characteristics of the Meteorological stations. The author then enters upon a careful discussion of the observations obtained in 1875, under the heads above mentioned.

We cannot take leave of this most interesting volume without

feeling assured that a great meteorological work has been commenced in India with every prospect of its being carried on successfully, so that in a few years it will rank with the great American and Mauritius systems, the one gathering and disseminating meteorological data over the North-American continent, the other working up the meteorology of the Indian Ocean most advantageously, for the benefit of the numerous vessels traversing its surface. We are not unmindful of the labours in this direction of Piddington, who effected for the Bay of Bengal and the China Sea what Meldrum has for the Indian Ocean; and we look forward with confidence for some important and valuable contributions on the storms that visit the Bay of Bengal in future volumes of the ' Indian Meteorological memoirs. Indeed the meteorology of India will not be complete without a *résumé* of the storms which have visited India and the LAWS deducible from them; and we are satisfied, from the Reports before us, that India possesses men fully equal to the work.

The Theory of Sound. By JOHN WILLIAM STRUTT, Baron RAYLEIGH, *M.A., F.R.S., formerly Fellow of Trinity College, Cambridge.* Vol. I. London : Macmillan, and Co. (8vo. Pp. 326).

This is the first volume of a work of great scientific importance. Its object is to supply the student with a complete view of the mathematical treatment of the subject; to do for it in its present stage of development what was done for it as it stood forty years ago by Sir J. Herschel's " well-known article on Sound " in the ' Encyclopædia Metropolitana.' In the present volume the author does not get so far as to treat of Atmospheric vibrations. So elaborate a treatment of the vibrations of other bodies, it might be thought, would turn the treatise into a general treatise on wave-motions ; its limits, however, are determined by a sort of common sense. But it will be best to let the author speak for himself on this subject.

" In the choice of topics to be dealt with in a work on Sound, I have for the most part followed the example of my predecessors. To a great extent the theory of Sound, as commonly understood, covers the same ground as the theory of Vibrations in general ; but unless some limitation were admitted, the consideration of such subjects as the Tides, not to speak of Optics, would have to be included. As a general rule we shall confine ourselves to those classes of vibrations for which our ears afford a ready-made and wonderfully sensitive instrument of investigation." (P. vi.)

Of the ten Chapters comprised in the present volume, the first three are introductory. The *first* gives a brief view of the leading facts concerning the propagation of Sound, and those relating to musical notes and tones ; the *second* treats of harmonic motions kinematically ; and the *third* discusses very fully the case of a vibrating body having one degree of freedom. The next two Chapters (the *fourth* and *fifth*) are devoted to the consideration of vibrating systems in general ; the last five to the special systems of Strings, Bars, Membranes, and Plates.

For the purpose of giving some notion of the extent to which these subjects are treated, we will indicate briefly the contents of one chapter; and for this purpose we will take the last, viz. that on Vibrations of Plates—a plate being a thin solid " of uniform iso-tropic material and constant thickness " (p. 293). The general expressions in the case of such a plate are first investigated for the potential energy of each unit of area, and for its variation from which the equation for the motion of the plate at any one point is found, and then the equations of condition arising from the state of its boundary, whether free, clamped, or supported. In subjects of this kind, however, the difficulty only begins when the general equations have been formed; and accordingly the next step is to modify them to suit the case of a circular plate and to integrate them when thus modified. The results obtained are compared— both in respect to the principal tones, and the nodal lines—with the results of observation; and a sketch is given of the history of the problem. Two other cases are also discussed. The first is that of a rectangular plate whose edges are free (the case in which the edges are supported being but briefly noticed); but in this case the mathematical difficulties necessitate the supposition that $\mu = 0$; *i. e.* the lateral contraction is assumed to be evanescent in comparison with the longitudinal extension. The results ob-tained on this supposition as to nodal lines and principal tones are found to admit of pretty close comparison with observations on a square plate. The second case is that of a cylinder or ring.

This statement will, perhaps, serve to convey some notion of our author's treatment of the several special systems of vibrating bodies. Of the contents of Chapters 4 and 5, which treat of vibrating sys-tems in general, it is not easy to write without going into details such as our limits will not allow. Partly this is due to the ex-treme generality of the statements : *e. g.* such a statement as this— A force of any type acting alone produces in a system a displace-ment of a second type from the zero configuration equal to a dis-placement of the first type due to the action of an equal force of the second type—is scarcely intelligible apart from the reasoning by which it is proved, though a particular case mentioned by way of illustration is plain enough :—" If A and B be two points of a rod supported horizontally in any manner, the vertical deflection at A when a weight W is attached at B, is the same as the deflection at B when W is applied at A " (p. 69). So, again, " Young's Theorem " (p. 144) is perfectly intelligible as a simple statement, but in its generalized form (p. 99) it is almost unintelligible without its context.

One important point in these chapters may be mentioned, viz. the introduction into the Equations of Motion of a function (F) called the " dissipation function," to represent the forces arising from friction and viscosity, F being a homogeneous quadratic function of the velocities. The author, it must be added, has done every thing that could well be expected to smooth down the asperi-ties of a very difficult subject, both in the way of illustration and

example, and particularly in regard to the difficulties incident to
the treatment of " Vibrating Systems in general." He has done
this by his elaborate discussion of the case of a vibrating system
having one degree of freedom, which, as already mentioned, forms
the subject of Chapter 3, as well as by the discussion of the system
having two degrees of freedom at the end of Chapter 5.

The complaint has often been made, and with regard to widely
different subjects, that, with a few conspicuous exceptions, our
best scholars and ablest men of science do not write—that they
content themselves with the pleasant task of acquiring knowledge,
and possibly of adding to it by means of brief memoirs which are
apt to be lost in the waste sea of the literature of Memoirs and
Periodicals. The last step between acquiring the knowledge, and
drawing up a formal statement of it for the benefit of others in-
volves labour which they decline to take*. The present volume is
a striking exception to the common practice. Its noble author
might well have considered himself absolved from the irksome labour
of writing a book, a task which he might have regarded as falling
more properly to the lot of the professional mathematician. We
do not doubt that this consideration will add to the gratitude of
students, who will find in the work before us a means by which their
labours in this branch of science will be most materially lightened.

XII. *Proceedings of Learned Societies.*

ROYAL SOCIETY.

April 26, 1877.—Dr. J. Dalton Hooker, C.B., President, in the
Chair.

THE following paper was read :—
    " On Repulsion resulting from Radiation.—Preliminary Note
on the Otheoscope." By William Crookes, F.R.S. &c.

I communicated to the Royal Society in November last an
account of some radiometers which I had made with the object
of putting to experimental proof the " molecular pressure " theory
of the repulsion resulting from radiation. Continuing these re-
searches, I have constructed other instruments, in which a movable
fly is caused to rotate by the molecular pressure generated on
fixed parts of the apparatus.

In the radiometer, the surface which produces the molecular
disturbance is mounted on a fly, and is driven backwards by the
excess of pressure between it and the sides of the containing vessel.
Regarding the radiometer as a heat-engine, it is seen to be im-

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* "Not the least of the many benefits which he conferred was the example
he set of unceasing labour ; for this was a permanent rebuke to that indolence
which is the besetting failing of the place—not the grosser form of aimless waste
of time, but the more seductive error which consists in the mere acquisition of
knowledge, which is never reproduced for the benefit of others."—Todhunter's
Account of the Writings of W. Whewell, vol. i. pp. 415, 416.

perfect in many respects. The black or driving surface, corresponding to the heater of the engine, being also part of the moving fly, is restricted as to weight, material, and area of surface. It must be of the lightest possible construction, or friction will greatly interfere with its movement; it must not expose much surface, or it will be too heavy; and it must be a very bad conductor of heat, so as to retain the excess of pressure on one side. Again, the part corresponding to the cooler of the engine (the side of the glass bulb) admits of but little modification. It must almost necessarily be of glass, by no means the best material for the purpose; it is obliged to be of one particular shape; and it cannot be brought very near the driving surface.

A perfect instrument would be one in which the *heater* was stationary; it might then be of the most suitable material, of sufficient area of surface, and of the most efficient shape, irrespective of weight. The *cooler* should be the part which moves; it should be as close as possible to the heater, and of the best size, shape, and weight for utilizing the force impinging on it. By having the driving surface of large size, and making it of a good conductor of heat, such as silver, gold, or copper, a very faint amount of incident radiation suffices to produce motion. The black surface acts as if a molecular[*] wind were blowing from it, principally in a direction normal to the surface. This wind blows away whatever easily movable body happens to be in front of it, irrespective of colour, shape, or material; and in its capability of deflection from one surface to another, its arrest by solid bodies, and its tangential action, it behaves in most respects like an actual wind.

Whilst the radiometer admits of but few modifications, such an instrument as the one here sketched out, is capable of an almost endless variety of forms; and as it is essentially different in its construction and mode of action to the radiometer, I propose to identify it by a distinctive name, and call it the Otheoscope ($\dot\omega\theta\dot\epsilon\omega$, I propel).

The glass bulb is an essential portion of the machinery of the radiometer, without which the fly would not move; but in the otheoscope the glass vessel simply acts as a preserver of the requsite amount of rarefaction. Carry a radiometer to a point in space where the atmospheric pressure is equal to, say, one millimetre of mercury, and remove the glass bulb; the fly will not move, however strong the incident radiation. But place the otheoscope in the same conditions, and it will move as well without the case as with it.

In the preliminary note already referred to [†], I described a piece of apparatus by which I was able to measure the thickness of the layer of molecular pressure generated when radiation impinged on a

[*] *Molecular,* not *molar.* There is no wind in the sense of an actual transference of air from one place to another. This molecular movement may be compared to the movement of the gases when water is decomposd by an electric current. In the water connecting the two poles there is no apparent movement, although eight times as much matter is passing one way as the other.
[†] Proc. Royal Soc. Nov. 16, 1876, p. 310.

blackened surface at any degree of exhaustion. At the ordinary density of the atmosphere the existence of this molecular disturbance was detected several millimetres off, and its intensity increased largely as the generating surface and movable plate were brought closer together. It would be possible, therefore, to construct an otheoscope in which no rarefaction or containing vessel was necessary, but in which motion would take place in air at the normal density *. Such a heat-engine would probably work very well in sunlight.

Aided by the mechanical dexterity of my assistant, Mr. C. H. Gimingham, I have constructed several varieties of otheoscope. These will be exhibited at the Soirée of the Royal Society on Wednesday next, as illustrations of the very beautiful manner in which, at this stage of my investigations, theory and experiment proceed hand in hand, alternately assisting each other, and enlarging our knowledge of those laws of molecular movement which constitute a key to the relations of force and matter.

The following is a list of the otheoscopes I have already made, together with some new experimental radiometers, which will be exhibited for the first time on Wednesday :—

1. *Otheoscope.*—A four-armed fly, carrying four vanes of thin clear mica, is mounted like a radiometer in an exhausted glass bulb. At one side of the bulb a plate of mica blacked one side is fastened in a vertical plane, in such a position that each clear vane in rotating shall pass the plate, leaving a space between of about a millimetre. If a candle is brought near, and by means of a shade the light is allowed to fall only on the clear vanes, no motion is produced; but if the light shines on the black plate, the fly instantly rotates as if a wind were issuing from this surface, and keeps on moving as long as the light is near.

2. *Otheoscope.*—A four-armed fly carries roasted mica vanes, and is mounted in an exhausted glass bulb like a radiometer. Fixed to the side of the bulb are three plates of clear mica, equidistant from each other in a vertical plane, but oblique to the axis. A candle brought near the fixed plates generates molecular pressure, which, falling obliquely on the fly, causes it to rotate.

3. *Otheoscope.*—A large horizontal disk revolving by the molecular disturbance on the surface of inclined metallic vanes, which are blacked on both sides in order to absorb the maximum amount of radiation.

4. *Otheoscope.*—Inclined aluminium vanes driven by the molecular disturbance from the fixed black mica disk below, blowing (so to speak) through them.

5. *Otheoscope.*—A large horizontal coloured disk of roasted mica, driven by inclined aluminium vanes placed underneath it.

6. *Otheoscope.*—A bright aluminium disk cut in segments, and each segment turned at an angle, driven by a similar one below of lampblacked silver.

* Since writing this I have constructed such an instrument. The movement takes place in the way I had anticipated.—W. C., April 26th, 1877.

7. *Radiometer.*—A vertical radiometer, made with eight disks of mica blacked on one side, and the whole suspended on a horizontal axis which works in two glass cups. The motion of the radiometer is assisted on each side by driving vanes of aluminium blacked on one side.

8. *Radiometer.*—A vertical turbine radiometer, the oval vanes of roasted mica blacked on one side.

9. *Radiometer.*—A spiral radiometer of roasted mica blacked on the upper side.

10. *Radiometer* of large size, showing great sensitiveness.

11. *Radiometer.*—A two-disk radiometer, the fly carrying roasted mica disks blacked on one side ; in front of each black surface is fixed a large disk of thin clear mica. The molecular disturbance set up on the black surface, and streaming from it, is reflected in the opposite direction by the clear plate of mica, causing the fly to move abnormally, *i. e.* the black surface towards the light.

12. *Radiometer.*—A two-disk radiometer, the fly carrying roasted mica disks blacked on one side, similar to No. 11, but with a large clear disk on each side. The molecular disturbance, prevented from being reflected backwards by the second clear disk, is thus caused to expand itself in a vertical plane, the result being a total loss of sensitiveness.

13. *Radiometer.*—A two-disk, cup-shaped, aluminium radiometer, facing opposite ways ; both sides bright. Exposed to a standard candle 3·5 inches off, the fly rotates continuously at the rate of one revolution in 3·37 seconds. A screen placed in front, so as to let the light shine only on the convex surface, produces repulsion of the latter, causing continuous rotation at the rate of one revolution in 7·5 seconds. When the convex side is screened off, so as to let the light shine only on the concave, continuous rotation is produced at the rate of one revolution in 6·95 seconds, the concave side being apparently attracted. These experiments show that the repulsive action of radiation on the convex side is about equal to the attractive action of radiation on the concave side, and that the double speed with which the fly moves when no screen is interposed is the sum of the attractive and repulsive actions.

14. *Radiometer.*—A two-disk, cup-shaped, aluminium radiometer, lampblacked on the concave surfaces. In this instrument the usual action of light is reversed, rotation taking place, the bright convex side being repelled, and the black concave attracted. When the light shines only on the bright convex side, no movement is produced ; but when it shines on the black concave side, this is attracted, producing rotation.

15. *Radiometer.*—A cup-shaped radiometer similar to the above, but having the convex surfaces black and the concave bright. Light shining on this instrument causes it to rotate rapidly, the convex black being repelled. No movement is produced on letting the light shine on the bright concave surface, but good rotation is produced when only the black convex surface is illuminated.

16. *Radiometer.*—A multiple-disk, cup-shaped, turbine radio-

meter, bright on both sides, working by the action of warm water below and the cooling effect of the air above.

17. *Radiometer.*—A four-armed, metallic radiometer with deep cups, bright on both sides.

18. *Radiometer.*—A four-armed radiometer, the vanes consisting of mica cups, bright on both sides.

19. *Radiometer.*—A four-armed radiometer having clear mica vanes, the direction of motion being determined by the angle formed by the mica vanes with the inner surface of the glass bulb.

GEOLOGICAL SOCIETY.

[Continued from vol. iv. p. 312.]

November 21, 1877.—John Evans, Esq., F.R.S., D.C.L., Vice-President, in the Chair.

The following communications were read :—

1. " On the Glacial deposits of West Cheshire, together with lists of the fauna found in the Drift of Cheshire and adjoining Counties." By W. Shone, Esq., F.G.S.

The conclusions arrived at by the author in this paper were as follows. Like Prof. Hull, he distinguished a triple division of the deposits under consideration. 1. The Lower Boulder-clay, or, as he preferred to call it, Lower Glacial Drift, resting immediately upon the eroded surface of the Keuper, consists for the most part of compact clay, containing numerous and large striated erratics, together with a fauna of Scandinavian type, the Gasteropoda being generally filled with fine silt containing Microzoa. The author believed that the shells found in this deposit were principally distributed by ground-ice, which took them up and floated them off the shore. 2. The Middle Sands and Gravels, or Interglacial Drift of the author, consist chiefly of sands and gravels containing few (if any) glaciated stones. The fauna of this division is Celtic, with a few Scandinavian species derived from the Lower Boulder-clay ; the shells were distributed principally by currents ; and the Gasteropoda were seldom, if ever, filled with sand containing Microzoa. 3. The Upper Boulder-clay, or Upper Glacial Drift, is composed for the most part of clay not so compact as the Lower Boulder-clay, and containing fewer and smaller glaciated stones, which are more abundant near the base. The fauna is Scandinavian at the base of the beds. The shells were distributed principally by ground-ice, and those of southern type derived from the Middle Sands and Gravels. The Gasteropoda are chiefly filled with silt containing Microzoa. The paper was accompanied by lists and tables of fossils, a large collection of which was exhibited in illustration of the paper.

[The Chair was then taken by Warington Smyth, Esq., M.A., F.R.S., F.G.S.]

2. " The Moffat Series." By C. Lapworth, Esq., F.G.S.

The fossils found in the highly convoluted Lower Silurian rocks of the southern uplands of Scotland are usually restricted to certain

narrow bands of black carbonaceous and Graptolitic shales, which, from their especial abundance in the neighbourhood of the town of Moffat, Dumfriesshire, are known to geologists as the Moffat Shales or Moffat Series.

The most perfect section of the black shales visible within the Moffat area is exhibited in the cliffs of the gorge of Dobb's Linn, at the head of Moffatdale. It was shown by the author that they are here disposed in a broken and partially inverted anticlinal, which throws off on both sides the basal beds of the surrounding non-fossiliferous greywackes. They are distinctly arranged in three successive groups or divisions. Each of these divisions is distinguished by special lithological characteristics, and possesses a distinct fauna. To the lower and middle divisions a few fossils are common ; but between the middle and upper divisions the palæontological break is complete. These divisions, again, are naturally subdivided into several zones, each characterized by special species or groups of species.

A larger exposure of the same deposits occurs at Craigmichan, a few miles to the south-west, where the beds of the lower division are shown to a much greater depth than at Dobb's Linn. In these two localities the general succession of the Graptolitic shales is as follows :—

			feet.
(c) *Birkhill Shales,* or Upper Moffat.	(b) Upper Birkhill	{ Grey and purple flagstones, with lines of black and white shale.	70 to 80
	(a) Lower Birkhill	{ Black pyritous shales, with seams of brightly coloured clays......	60 to 70 .
(b) *Hartfell Shales,* or Middle Moffat.	(b) Upper Hartfell	{ Pale grey or green non-fossiliferous mudstones	45
	(a) Lower Hartfell	Black hard slaty shales and flags	40 to 50
(a) *Glenkiln Shales,* or Lower Moffat.		{ Yellow and grey shales and flags, non-fossiliferous, with a few bands of soft black Graptolitic shales	150 .

With the aid afforded by these sections, the thorough investigation of the ten subparallel black shale-bands of the Moffat area is rendered a matter of ease and certainty. Of these, the four bands lying to the south-west of Saint Mary's Loch are the most continuous. They were described in detail by the author ; and it was shown that in each the only strata apparent are indisputably those of the type sections of Dobb's Linn and Craigmichan, with which they agree zone for zone in sequence and in all their characters, mineralogical and zoological. Here, also, the beds are arranged in greatly elongated anticlinal forms, the axes of which are, as a rule, inverted. In any single transverse section, the succession of the beds on the opposite sides of the median line of the band is identical ; and the highest zone of the black shales everywhere passes up conformably into the basal bed of the surrounding greywackes. The varying width of the band is dependent simply upon the varying elevation of the crown of the anticlinal. Where the band is of least diameter, only the highest beds of the Birkhill shales rise from below the greywackes. As the band expands, the underlying zones

emerge one by one in its centre, till finally, in the widest exposures, we recognize the deepest strata of the Glenkiln shales.

It was shown, by plans, sections, and descriptions of every expo-sure of consequence within the Moffat district, that precisely similar results are arrived at with respect to the remaining black shale-bands. To the south of Moffatdale, the Moffat beds agree essentially with those of Dobb's Linn ; but to the north the whole formation diminishes in collective thickness, and the highest division gradually loses its fossiliferous black shales.

These facts place it beyond question that all the carbonaceous and Graptolitiferous shales of the Moffat area are portions of one and the same originally continuous deposit—the *Moffat Series*, which is now the oldest visible rock-group in the district, being everywhere in-ferior to the prevailing greywackes, through which it invariably rises from below in greatly elongated anticlinal forms.

In the rigid restriction of distinct groups of fossils to a few feet of the succession, the rocks of the Moffat series resemble the thin-bedded Silurians of Scandinavia and North-eastern America. From analogy it may be suspected that they similarly represent an enor-mous period of time. The correctness of this inference is demon-strated by the evidence afforded by the known geological range of their organic remains. The Graptolithina of the Lower or Glenkiln division are those of the highest Llandeilo Flags of Wales, the corresponding Middle *Dicranograptus*-schists of Sweden, and the Norman's-Kiln shales that *underlie* the Trenton (Bala) Limestone of New York. The Hartfell species occur in the Bala beds of Conway, &c., the higher *Dicranograptus*-schists of Sweden, and the Utica and Lorraine shales that *overlie* the Trenton Limestone. Those of the Birkhill shales agree almost species for species with the fossils of the Coniston Mudstone of Cumberland, the Kiesel-Schiefer of Thuringia, and the Lobiferous beds of Sweden, which lie at the summit of the Lower Silurians of their respective countries. Hence it may be considered certain that the Glenkiln shales are of highest Llandeilo age, that the Hartfell shales stand in the place of the Bala or Caradoc of Siluria, and that the Birkhill shales correspond to the Lower Llandovery.

The insignificant thickness of these three formations in the Moffat district is in strict agreement with the well-known north-westerly attenuation of the Lower Silurian rocks in Wales, England, and in Western Europe generally.

It was pointed out that these results, when carried to their legitimate conclusion, harmonize all the apparently conflicting facts hitherto collected among the Lower Silurians of the south of Scot-land. We have a complete explanation of such difficulties as the remarkable lithological uniformity of the predominating strata, the absence of associated igneous rocks, the peculiar localization of the fossils, their identity along certain lines, and their rapid and peculiar impoverishment along others. We reduce, at a single stroke, the apparently gigantic thickness of the South Scottish Silurians to reasonable limits, and at the same time bring them into perfect harmony with those of Western Europe and America.

XIII. *Intelligence and Miscellaneous Articles.*

ON THE COMPOSITION AND THE INDUSTRIAL USE OF THE GASES
ISSUING FROM METALLURGIC HEARTHS. BY L. CAILLETET.

THE remarkable investigations of H. Sainte-Claire Deville on dis-
sociation, in opening to science a new path of research, have
likewise promised to interpret a great number of metallurgical
phenomena which had till then remained unexplained.

By collecting the gases which circulate in the hottest part of the
furnaces in which iron is worked, I have been able, by means of
apparatus similar to M. Deville's, to prove that the composition of
those gases, suddenly cooled, is totally different from the results
given by the analyses of Ebelmen. That skilful metallurgist, unac-
quainted with the phenomena of dissociation, collected the gases by
slowly aspirating them by means of a long tube—which necessarily
brought about the combination of their dissociated elements.

In Ebelmen's analyses the reaction seems almost always com-
plete, while the cooling undergone by the gases shows that smoke
and carburetted gases can subsist in presence of oxygen at the tem-
perature of welding iron.

The gases collected at the top of the grating of an annealing-oven,
at a point where the temperature is such that the eye cannot sup-
port the brightness of the bricks raised to a most intense whiteness,
contain :—

Oxygen	13·15
Carbonic oxide	3·31
Carbonic acid	1·04
Nitrogen (by difference)..	82·50
	100·00

Independently of the carbonic oxide, there is found in the oxi-
dizing atmosphere of the oven a large excess of finely divided carbon,
which deposits itself on the tube, *hot* and *cold*, which serves for the
aspiration.

In metallurgic works the gases issuing from welding-fires are
generally conducted beneath generators, which thus produce with-
out expense the supply necessary for the working of engines. The
gases, therefore, rapidly cool against the walls of the boiler; thus,
after traversing a length of 15 metres, their temperature is below
500°. They are then formed of

Oxygen	7·65
Carbonic oxide	3·21
Carbonic acid	7·42
Nitrogen (by difference)..	81·72
	100·00

It may be concluded from this analysis that the quantity of
oxygen has diminished by nearly one half, in reacting, not upon
the carbonic oxide, of which the proportion has changed but little,
but upon the finely divided carbon, which exists in large quantity,

as I have shown, in the atmosphere of the hearth. The cooling and extinction of the gases stops all reaction; and when the latter are thrown off by the chimney they still contain, as we see, large quantities of combustible materials.

The investigations which I have made for the purpose of taking up a portion of these gases, left hitherto unused, have demonstrated that it is easy to rekindle them by passing them over a fire, at the same time retarding their motion. It was with this view that, in my forges at Saint-Marc (Côte d'Or), I had a furnace of large dimensions set up to receive the gases as they issued from the generator. On arriving in this furnace, the section of which is more than 3 square metres, the gases lose a large portion of their velocity, at the same time that they are kindled in passing over a small grating on which coal-cinders, or some combustible of small value, are burned.

The high temperature developed in these conditions is utilized in my works for the annealing of sheet-iron. It is, in fact, known that rolling renders the iron brittle, and that it becomes covered with adherent oxide in the annealing-ovens. By heating the sheets thus altered for twelve hours in cast-iron boxes well closed, arranged in the gas-oven just mentioned, the sheets are found, after complete cooling, to have become perfectly malleable; and the oxide has disappeared, leaving the surfaces clean and bright. This reduction is easily explained if we remember the beautiful researches of MM. H. Sainte-Claire Deville and Troost on the passage of hydrogen through red-hot metals. I have likewise had the honour to communicate to the Academy* various experiments which prove that, on plunging a flattened iron tube into a fire, hydrogen passes through its sides, and, accumulating within it, causes it to resume its original form. The gases which have penetrated into the cast-iron box under the influence of the red-hot sides are therefore essentially reducing, and produce in a very short time complete deoxidation of the metallic surfaces.

In brief, we may conclude from my experiments :—

1. That the gases issuing from metallurgic fires still contain, even after passing under steam-generators, an important quantity of combustible principles, and that, with the aid of the processes above described, it is easy to kindle them afresh and burn them almost completely.

2. That the passage of reducing gases through the red-hot metallic walls is capable of receiving applications in metallurgy which doubtless will not be limited to the particular case of which I have given an account.—*Comptes Rendus de l'Académie des Sciences,* Nov. 19, 1877, tome lxxxv. pp. 955–957.

ON A PILE IN WHICH THE ATTACKABLE ELECTRODE IS OF COKE.
BY P. JABLOCHKOFF.

The coke burned in steam-engines produces work which, trans-

* *Comptes Rendus,* t. lviii. pp. 327, 1057.

formed into electricity by means of magneto-electric machines, supplies this electricity much more economically than any chemical-action pile that has hitherto existed. This consideration gave me the idea of producing electricity by attacking coke directly. But, as every one knows, coke is not attacked by any liquid at ordinary temperatures; I was therefore obliged to construct an electro-chemical pile with hot liquid.

Now it was evident that bodies which are liquid at ordinary temperatures would be vaporized at the temperature necessary for attacking coke. Hence it was necessary to take a substance which would only become liquid at a sufficiently elevated temperature, and be converted into vapour only at a very high temperature.

With this view I fused either nitrate of potass or nitrate of soda; and in this liquid I immersed as attackable electrode ordinary coke, and platinum as the unattackable electrode. But experience has proved to me that this latter electrode may be iron, cast iron, or any other metal which in the presence of coke is not attacked by the liquid.

By adding different metallic salts one can vary the electromotive force of the pile, the velocity of combustion of the coke; and with those salts the galvanoplastic deposit of the metals is received upon the unattackable electrode.

The electromotive force of the pile varies between 2 and 3 units, according to the nature of the metallic salts introduced into the liquid; this force is therefore superior to that of either the Bunsen or the Grenet pile; indeed the Bunsen pile gives the maximum of 1·8 unit, the Grenet pile 2 or, in the most favourable conditions, 2·1 units.

To set the pile in action in the most practical manner, it is not necessary to fuse the alkaline nitrate beforehand; it suffices to ignite a piece of coke and put it in contact with the nitrate in powder. Chemical action commences immediately; the temperature produced fuses the salt which surrounds the coke; and the pile enters upon its functions. During the activity of the pile much carbonic acid and other gases are liberated. I have devised an arrangement permitting the gas to be stored, in order to make it serve as a motive power. The following is the practical arrangement of the elements of the pile :—

A cast-iron pot, of a cylindrical shape, serves at the same time as receiver and unattackable electrode. An iron-wire basket, of concentric form, serves for holding the coke, and at the same time plays the part of a rheophore. As the coke and fused salt are consumed, fresh quantities of both substances can be added by hand, or the pile can be fed automatically, during the whole time of the operation. Contrary to what might have been thought, the combustion is not at all rapid.

Therefore, by this process, direct combustion of coke gives the electric current, the deposition of metals, and a motive power.— *Comptes Rendus de l'Académie des Sciences,* Dec. 3, 1877, tome lxxxv. pp. 1052, 1053.

ON THE LAW OF ABSORPTION OF RADIATIONS THROUGH BODIES,
AND ITS EMPLOYMENT IN QUANTITATIVE SPECTRAL ANALYSIS
(PART I.).   BY G. GOVI.

When we interpose an absorbent medium in the path of the
white light which passes through the slit of a spectroscope, we
usually see dark bands appear in different parts of the spectrum,
which there diminish the brightness of the colours or even com-
pletely extinguishes them.   It is seldom that these bands do not
invade a great number of contiguous wave-lengths, which they ob-
scure in spreading more feebly on both sides of a more intense line
of absorption.   If the thickness of the absorbent medium be aug-
mented, fresh shaded bands often appear between the former ones ;
but what never fails to be produced is the strengthening of the
first bands and their progressive dilatation ; so that, for a certain
thickness of the medium, the entire spectrum is invaded by the
shade, and so much enfeebled that it may be regarded as quite ex-
tinguished.

This progressive widening of the absorption-bands singularly
reminds one of the increase in number and the dilatation of the
bright lines which several observers have verified in the spectra of
incandescent gases in proportion as their rarefaction is diminished
and their temperature augmented ; so that it is quite possible the
two phenomena may correspond and be complementary the one to
the other for one and the same substance.   Moreover all the radia-
tions, visible and invisible, of the spectrum present analogous phe-
nomena ; and if we here speak of the luminous radiations only, it is
solely because their study is much more convenient and more usual
than that of the ultra-red or ultra-violet radiations.

It is obvious, from what has just been said, that the absorptive
power of a substance is not sufficiently characterized by such or
such a dark band appearing in the spectrum of the white light
which has passed through a certain thickness of it, and that, in
order to define it perfectly, we must know all the modifications it
can determine in the spectrum, from the slightest and most limited
up to that which produces sensible extinction of all the radiations.
In other words, we do not truly know the absorptive power of a
substance unless we have determined its coefficients of absorption
for all the wave-lengths that can be studied, from those correspond-
ing to obscure heat to those met with at the limit of photogenic
action.

This is why Sir J. Herschel, and many others after him, have
attempted to construct, through points, the curves which were to
express the values of the absorption-coefficients, as functions of
the wave-lengths, for different bodies ; but the discontinuity of the
artifices employed, and the absence of every photometric measure,
have hitherto permitted only very incomplete results to be obtained.
It is nevertheless not impossible to obtain a more rigorous defini-
tion of the absorptive power of bodies, either by making directly
apparent to sight the curves themselves of equal chromatic absorp-

tion in their entire development, or by measuring the luminous intensity along the whole extent of the spectrum in order afterwards to deduce therefrom the corresponding coefficients of absorption. It suffices for observing directly the spectral curves of equal absorption *, to give to the absorbent the form of a prism (or, more strictly speaking, of a wedge) more or less acute, which is applied by one of its plane faces close to the slit of a spectroscope. The edge of this prism is placed at one end of the slit, parallel to its width; and we find that we thus have, over the entire length of the opening, an absorbent medium, the thickness of which increases uniformly from zero (at the edge) up to a certain maximum depending on the angle of the prism and the length of the slit.

The deviation due to the prismatic form of the medium is to be destroyed by opposing to the first a second prism of a material as little absorptive as possible (rock-salt, fluor-spar, quartz, glass, water, alcohol, &c.). The angle of this second prism is easily calculated approximately, which is sufficient in most cases; but it can at need be rendered variable within limits sufficiently wide for giving at all times almost perfect compensation.

When in this way an absorbent medium of variable thickness is placed in front of the slit of a spectroscope illuminated with perfectly white light (that of incandescent solids), the spectrum no longer appears, as usually, uniformly luminous throughout its height, but in it shades are distinguished more or less undulated or toothed, which exhibit immediately to the eye the law according to which the absorption-coefficient of the medium varies with the wave-length of the incident light.

These curves can be constructed by drawing them with the camera lucida, by fixing them by photography, or by referring them to two rectangular axes by aid of two luminous micrometers seen by reflection—one fixed parallel to the length of the spectrum, the other movable and normal to the first. All these means, however, of constructing the curves of chromatic absorption suppose that it is possible to recognize in them the points of equal intensity, which is not very easy; but it is useful to have recourse to them to represent the complete form of the law of absorption when we have to do with sufficiently absorbent substances and when rigorously exact measurements are not indispensable.

If the slit be divided into two parts in the direction of its length, and each of the two halves be employed for producing a spectrum with curves of chromatic absorption, the two spectra being juxtaposed in the direction of their length, nothing will be easier than to compare their curves and to ascertain the equality or the differences between them. We might even, by a tolerably simple artifice, slide one of two spectra, of one and the same absorbent material at two different degrees of concentration, over the other, ascertain the zones of equal intensity, and thus apply the spectroscope to the proportioning of such substance.

* "Metodo per determinare le curve spettrali d'assorbimento della luce nei varii mezzi," di Gilberto Govi (*Notizia storica dei lavori, ecc., dell'Accademia di Torino negli anni* 1864 *e* 1865, adunanza dell' 8 maggio 1864).

The employment of solar light permits the reference of the absorption-curves to the Fraunhofer lines, and consequently to the wave-lengths of the different points in the spectrum. If the prisms were replaced by networks, we should have a simpler representation of the relation connecting the coefficients of absorption with the different wave-lengths. When, however, we wish to study, with respect to chromatic absorption, substances endowed with a very feeble absorptive power, or desire to express more precisely the law of extinction for all the radiations, the process just described is scarcely suitable. In this case photometric means must be used, and recourse be had to the law of monochromatic absorption admitted by physicists for interpreting the results.— *Comptes Rendus de l'Académie des Sciences*, Dec. 3, 1877, tome lxxxv. pp. 1046–1049.

THE LIQUEFACTION OF OXYGEN.

One of the most interesting experiments in physics of our times has just been performed at Geneva, with rare success, in the works of the Physical-Instrument Manufacturing Company. Our fellow citizen, M. Raoul Pictet, has succeeded in obtaining, by means of ingeniously combined apparatus, the liquefaction of oxygen gas. The following are, briefly, the principles by aid of which this important result has been obtained :—By a double circulation of sulphurous and carbonic acids the latter gas is liquefied at a temperature of 65 degrees of cold under a pressure of 4–6 atmospheres. The liquefied carbonic acid is led into a tube of 4 metres length ; two combined-action pumps produce a barometric vacuum over this acid, which solidifies in consequence of the difference of pressure. Into this first tube (containing, as just said, solidified carbonic acid) passes a narrower tube, in which circulates a current of oxygen, produced in a generator containing chlorate of potass, and having the form of a large howitzer-shell with walls thick enough to prevent all risk of explosion. The pressure may be carried as far as 800 atmospheres. Yesterday morning, all the apparatus being arranged as just indicated, under a pressure not exceeding 300 atmospheres, a liquid jet of oxygen spirted from the end of the tubes at the moment when the compressed and refrigerated gas was passing from this high pressure to the atmospheric. What gives to this fact its great scientific interest is, that it experimentally demonstrates the truth of the mechanical theory of heat, by proving that all gases are vapours, capable of passing through the three states—solid, liquid, and gaseous. A fortnight since, M. Cailletet had succeeded in liquefying nitric oxide, under a pressure of 146 atmospheres, and at 11 degrees of cold. After the experiment of M. Raoul Pictet there remain only two gases that have as yet escaped the test of liquefaction—hydrogen and nitrogen. The fine experiment above described will, we are informed, be repeated on Monday next, and following days, with some slight changes in the processes and arrangement of apparatus.—*Journal de Genève*, December 23, 1877.

XIV. *An Account of some Experiments on Rigidity produced by Centrifugal Force.* * *By* John Aitken, *F.R.S.E.*†

[Plates III.–VII.]

THE experiments do not contain much that is new, many of the problems having previously been mathematically wrought out by Sir William Thomson and others. They, however, help to fill up a very evident gap in our experimental dynamics ; and it is hoped they will enable the general reader to form a clearer idea of the action of the so-called centrifugal force, a force the fundamental action of which is often mis-understood. This tendency to misunderstanding has arisen principally from two causes :—the one being the paucity of our experimental illustrations of centrifugal force (almost all our experiments only illustrate the action of this force round one centre and in one plane); the other cause being the name which has been applied to this force, the word centrifugal meaning "to fly from the centre," which has given rise to the idea that there is a force acting on a body when revolving round a centre tending to cause it to fly away from that centre. If some word more correctly expressive of the action of this force could be introduced, it would do much to remove the present confusion. Professor James Thomson, in a paper before the British Association at Glasgow in 1876, objects to the word centrifugal, and only retains it on account of its very

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* [The experiments were in part communicated to the Royal Society, Edinburgh, and also in part to the Philosophical Society of Glasgow, during the session 1875–76, and slight accounts published in the Proceedings of those Societies. A complete account, with drawings of the apparatus, is now published for the first time.—W. T.]

† Communicated by Sir W. Thomson, Dec. 1877.

general use. He has, however, some time ago suggested, and now generally uses, the word centreward in place of centripetal. It is much to be hoped that in his reforms he will not spare the word centrifugal, but will replace it by some word more etymologically correct. While objecting to the name centrifugal force, we are under the necessity of retaining it till some better term has been introduced. It will, however, be necessary for us clearly to understand what we mean by centrifugal force. According to the First Law of Motion, any body when in motion tends to move at a uniform velocity and in a straight line. If we wish the body to move in a circular or any curved path, then we must cause some force to act on it to compel it to deviate from its free path; and *the resistance which the body offers to this deviation is what we call centrifugal force.* Or, more simply, centrifugal force is the resistance which a body offers when in motion to change of direction of motion. If we remove the deviating force, then there is no centrifugal force, and the body simply tends to continue moving in a straight line*.

Suppose now that, instead of one body revolving round a centre, we have a number of bodies of the same mass, all moving at the same velocity, all placed at equal distances from each other and at equal distances from the centre, then we may cause this series of bodies to revolve round the centre by tying them to the centre, when they will exert a radial tension; or we may cause them to revolve round the centre by linking all the bodies together like a chain. When such a series of bodies are in motion round a centre, they exert a pressure at right angles to the direction of their motion, the result of which is, a tension is produced in the system tending to burst the links, in the same manner as the tension is produced in the shell of a cylindrical boiler by the pressure of the steam. According to the dynamical theory of gaseous pressure, these two tensions are produced in a very similar manner—in the first by the resistance to change of direction of motion of the links, and in the second by the resistance to change of direction of motion of the molecules of the steam.

The tension produced in a series of bodies revolving round a centre is very simply illustrated by taking an elastic band and fitting it tightly over a pulley which can be driven at a great velocity, such as that shown in Pl. IV. fig. 1, when it will be seen that as the velocity increases the tightness of the

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* For simplicity the body is here spoken of as a whole: and this is correct if the body is infinitely small; but if the body is of any size and moves round a centre either outside the body or inside it, then we must consider each particle of the body separately, as the different parts of the body are moving at different velocities and in different directions.

elastic band on the pulley diminishes, and if the velocity is sufficient it ceases to press on the pulley; and at length the tension produced by the centrifugal force opens out the band to such a size that it becomes larger than the pulley, its form and motion becoming irregular, and at last it flies off the pulley.

If all the bodies in the system are moving at the same velocity and in paths of the same curvature (that is, in a circle), then the tension and the centrifugal force will evidently be in equilibrium at all points, and the chain will keep its circular shape, because, as the rate of deviation is the same for all the bodies, the resistance of each of the bodies to this deviation (or what we call the centrifugal force) will be the same at all points, and the tension due to this resistance will also be the same.   The question may now be asked, Is this chain in a condition of stable or of unstable equilibrium ?   If the circular form were to be slightly destroyed, would the chain tend to return to the circular shape, or would it tend to depart further and further from it ?   Is the equilibrium of the chain the stable equilibrium of an egg resting on its side, or the unstable equilibrium of an egg balanced on its end ?   The answer we shall get to this question will be that the equilibrium of the moving chain corresponds to neither of these forms, but might be compared to the equilibrium of a perfectly spherical and homogeneous body resting on a perfectly horizontal plane, or floating in a fluid of the same specific gravity as itself, all positions being positions of equilibrium.

First, let us see what answer experiment gives to this question.   If we hang an endless chain over a pulley, and the pulley is caused to rotate at a great velocity, it has long been well known that the motion so communicated to the chain has but little tendency to alter the form of the curve in which the chain hangs, and that the principal effect of the motion is to confer on the chain a quasi-rigidity which enables it to resist any force tending to alter its curvature.   This statement must not, however, be taken as representing the facts of the case very accurately ; for while it may possibly be true of some ideal form of chain, yet I shall presently show that in all chains we can experiment on there are forces in action in the moving chain which cause it to depart from the form it had while at rest ; and if these forces were not balanced by gravitation, the form of the chain would soon become very different from what it was.

What some of these disturbing forces are I shall point out later on.   For the present we shall neglect them, as in most chains they are small, and shall simply consider the balance between the centrifugal force and the tension.   When the

chain moves in a circular path, the centrifugal force and the tension are evidently equal and balanced at all points ; and we also know from experiment that, when the chain hangs in the form of a long loop from a pulley, the tension just balances the centrifugal force at all points, as the chain has no tendency when in motion to alter the form of the loop. I shall not attempt to enter into a mathematical investigation of this balance between these forces ; my province is simply to describe some experimental illustrations ; I would, however, refer all those who wish for a mathematical investigation of the subject to Thomson and Tait's 'Elements of Natural Philosophy,' where they will find it fully treated. There is, however, an extremely simple geometrical demonstration, which I may venture to give before proceeding further.

Let us consider the equilibrium of an endless chain moving in a loop of such a form as that represented in fig. 7, Pl. VII., the links of which are moving in a path of varying curvature. As the velocity of the links is the same at all points, it will not be necessary for us to consider how different velocities of the links will affect the tension in the chain ; and the investigation confines itself to the consideration of the tension produced in the chain by the links when moving in paths of different rates and amounts of curvature.

The first point to be considered is, What is the effect of the rate of *curvature* of the chain when the angle of deflection is the same ? Suppose A B C and D E F, fig. 1, Pl. III., to be two chains moving with the same velocity and in directions parallel to each other, and suppose the radius of the curved part of the path in D E F to be only one half of what it is in A B C. Now, as the deflection is the same in both cases, the integral forces required to produce these deflections will evidently be equal; or (to state it in another way) the force required to destroy the momentum in the chain in the direction A B, and to generate momentum in a direction at right angles to A B, is the same in both cases. The whole resistance offered by all the links in the bend to this deflection, or what is called the centrifugal force, is therefore equal in both cases, and the tensions produced in the two chains will therefore be equal. So long, then, as the *deflection is the same* the *different rates of curvature* produce the same tension in the chain, and have no tendency to alter the path in which the chain moves. The only difference between the two cases is, that the centrifugal force in the curved part of the chain D E F is twice as great per unit of length as in the curved part of A B C. Suppose, for instance, that there are 10 links in the bend in D E F, and that each link exerts a force of 20 units. Then, as the radius

in the bend in A B C is twice as great as in D E F, there will be twice the number of links in A B C that there are in D E F, but the centrifugal force will only be one half what it is in D E F; so that in the bend in A B C we shall have 20 links each acting with a force of 10 units; the result of which is, the total force is the same in both cases, namely 200.

The next point to be considered is, What is the effect of the *amount of bend* or deflection on the tension of the chain? Suppose the chain to be bent to the angle A C B, fig. 2, Pl. III., what tension will it put on the chain?

Let C D = force required to destroy the whole momentum of the chain in the line A D.

Draw B E perpendicular to A D.   Join D B.

Then   E D = force required to destroy momentum at the bend in the line A D ;

and   E B = force required to establish momentum in the direction P Q ;

D B = total deviating force at C required to produce the bend A C B ;

B D = the centrifugal force at C in direction and magnitude.

On A C, C B construct the parallelogram A C B F. Join C F.

F C is equal and parallel to B D ;

∴ F C = the centrifugal force at the bend A C B.

It is evident from the construction that F C is always equal to B D (that is, always equal to the centrifugal force), whatever the angle A C B may be.

The centrifugal force F C acting at the angle A C B will produce a tension in the chain equal to C A or C B, because F C is equal and opposite to the resultant of C A and C B ; and, further, it is evident from the construction, that, while the angle or bend A C B may vary and the centrifugal force F C vary along with it, the tension C A or C B does not vary.

From these considerations we see that, though the centrifugal force in a chain moving in such a path as that shown at fig. 7, Pl. VII., varies at the different points, being greater the quicker and the greater the curvature, yet the tension produced by the centrifugal force at the different points is the same, and is independent of the *rate* or the *amount* of curvature, and that therefore the chain while in motion has no tendency to alter its shape.

These observations only refer to the tension in the chain produced by centrifugal force.   It is, however, necessary for us, in order to understand some of the results we shall presently see, to remember that there are other tensions in the chain besides this one. There is, for instance, the tension produced by

gravitation; this tension varies at different points, being nothing at the lowest point and increasing to a maximum at the highest point of the chain : and there is also the tension produced by putting and keeping the chain in motion; this tension is greatest at the part of the chain approaching the driving pulley, and least at the part just leaving it.

Description of Apparatus.

Before proceeding to the experiments, it will be necessary for me briefly to describe the apparatus used.   As chains were used in almost all the experiments, the object of the apparatus was simply to communicate in different ways motion to these chains ; the apparatus was therefore of the simplest description possible, and is represented to scale in figs. 1 to 5, Pl. IV. Fig. 1 is a general view of the principal part of the apparatus. A short steel spindle, a, running in a tube, is mounted horizontally on the triangular supports $b\,b$ at a convenient height for making the experiments ; on one end of the shaft, a, is an arrangement, c, for fixing on the different sizes and shapes of pulleys shown in fig. 2.   On the other end of the shaft is the small brass pulley d, $1\frac{7}{8}$ inch in diameter ; e is the driving-wheel made of wood, 2 feet in diameter, having a groove for a driving-cord cut in its circumference.   The driving-wheel, e, is fitted to the end of a horizontal axle running in the bearings, $f\,f$; the bearings are cast in one piece with the plate g, which is held firmly to the sole-plate h by means of the two screws $i\,i$, which pass through longitudinal openings in the plate g to admit of the wheel being moved in a longitudinal direction for the purpose of adjusting it to the proper distance from the pulley d to keep the driving-cord k tight*.   The driving-wheel is driven by means of the handle l ; and motion is communicated to the small spindle a by means of the cord k.   The driving-wheel is easily unmounted from its axle ; and the triangular supports are fixed to the sole-plate by means of two long bolts passing through both supports and clamping them firmly to the sole-plate.   The tube carrying the spindle a being also held by screws to the supports $b\,b$, the whole apparatus is easily unmounted for packing away.

The chain, n, to be experimented on is hung over the pulley A fitted to c on the end of the spindle a, and the pulley put into rapid motion by means of the handle l.   As the small shaft makes about 13 revolutions for one of the driving-wheel,

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* If a small india-rubber band, such as those sold by stationers, is stretched over the grove in the pulley d, the friction between the cord and the pulley is very greatly increased, and the tightness of the cord becomes a matter of less importance.

a velocity of from 40 to 70 feet per second may easily be communicated to the chains.

The pulleys for fitting to the spindle a are of different forms and sizes, and are shown to scale at A, B, C, D, fig. 2 ; their uses will be pointed out in describing the experiments.

Very light chains were used, as many experiments can be made with light chains which would be dangerous or impossible with heavy ones without more elaborate apparatus. The chains were almost all machine-made, as the weight per unit of length is much more uniform in machine- than in hand-made ones ; their motion is therefore much more regular. They were generally of the common figure-8 pattern, one half of the link being at right angles to the other half. The chains of this kind varied in weight from $1\frac{1}{2}$ oz. per yard to 10 oz. per yard ; other kinds of chains were also used, amongst them the pin and flat-link chain used for hanging windows.

For guiding and altering the shapes of the curves of the chains while in motion, wooden pulleys having grooves in their circumference, and running freely on steel spindles fixed to handles, were used. The shapes and sizes of these are shown at E, F, fig. 3. In some cases the hollow india-rubber ball shown at G, fig. 3, was found useful, not only for altering the shapes of the curves of the chain, but also for other purposes, which shall be described further on. The india-rubber ball was mounted on a steel spindle, by passing a brass tube through it, and fixing a circular brass plate to each end of the tube, and cementing the ball to the end plates. The india-rubber ball did very well at first; but as the chains require to be kept well oiled, the oil soon spoiled the india-rubber and made the ball nearly useless. The following plan was then adopted:—A flange about 2-inch diameter was fixed to the end of a short piece of brass tube, and a loose flange and nut was screwed on the other end ; disks of the required size were cut from sheet india-rubber, and holes pierced in their centres ; the brass tube was passed through the centres of a sufficient number of these disks ; the loose flange was then put on, and the whole screwed tightly up by means of the nut. This plan was adopted because the india-rubber could be easily and quickly replaced if destroyed. In place of india-rubber we may use disks of cloth. At first sight the pulley so constructed does not look very promising ; but after it is in use, the motion confers a quasi-elasticity upon it which enables it to do its work remarkably well.

For experiments with long chains the apparatus shown at fig. 4 was used. It is simply a short steel spindle, to one end of which a small pulley, a, is fixed, and to the other end the wooden pulley H ; the spindle bearings are fixed to a piece of

wood, *c*. The whole apparatus is firmly attached to a scaffolding, or any other convenient place, by means of screw-nails passing through *c*. The chain to be experimented on is hung over the pulley H; and an endless cord is passed over the pulley *a* and carried down to the driving-wheel of the apparatus shown in fig. 1, which may be standing on a table or other convenient place.

For experiments with a horizontal chain, a slight addition was made to the apparatus shown in fig. 1. These additions are shown in fig. 5. As before, *a* is the steel spindle, having the small brass pulley *d* fixed at one end, by which motion is communicated to it ; to the other end is fixed a wooden pulley *c*, having a groove in its circumference, in which is placed a stretched india-rubber band of circular section ; on the top of the pulley *c* rests the horizontal pulley *i*. Motion is communicated to the horizontal pulley *i* by means of the pulley *c*. The pulley *i* is supported in its position by means of the horizontal bars *e e* (only one of which is shown in sketch), which are clamped to the triangular supports *b b* by means of the screws *f f*. The horizontal bars *e e* are held together at the end by means of the cross-piece *g*, to which they are held by means of the screws *h h*. To the cross-piece *g* is clamped, by means of the screw *l*, the vertical bar of wood *m*; to *m* is attached, by means of the brass plate *p*, the tube *o*, in which runs the spindle of the pulley *i* ; the brass plate *p* moves in *m* round the screw *q* like a compass-joint. By this arrangement the pulley *i* may be fixed in a horizontal position, or turned to any angle that may be desired, by first turning *p* round the screw *q*, then lowering the bar *m* till the pulley *i* rests on the pulley *c*. The horizontal bars *e e* rest on pins, *u u u u*, driven into *b b*. The bars *e e* can be moved on the pins *u u u u* to the right or left, so as to cause the pulley *c* to act near the centre or the circumference of the underside of the pulley *i*, so as to give a quick or slow motion to the pulley *i*, as may be desired for the experiment. *r* is an india-rubber ball mounted on a steel spindle, and so arranged that it can be adjusted to press the chain against the pulley *i*, at whatever angle the pulley *i* is put. In order to increase the friction between the pulley *i* and the chain, the pulley is covered with india-rubber. The pulley *i* is pressed against the pulley *c* by means of a spiral spring at the lower end of the spindle *o* ; the spindle *o* is carried down below the horizontal bars *e e* for making experiments with apparatus hung from it. As all the bolts are provided with thumb-screws, the whole apparatus is easily and quickly taken to pieces.

In addition to the apparatus shown in Pl. IV., a simple arrangement was placed below the pulley A, fig. 1, for guiding

the chain to the groove in the pulley ; also an arrangement for fixing an india-rubber pulley in a position to press the chain against the pulley A : they were held in position by being clamped to the supports *b b.*

Description of Experiments.

I. If a long endless elastic band or cord is hung over the pulley fixed to the end of the spindle *a*, fig. 1, Pl. IV., and motion is gradually communicated to the band, the elastic band is seen to grow in length, the lower end of the loop getting further and further from the pulley, the loop keeping, however, much the same shape it had while at rest. This elongation becomes very marked if the band is loaded : if, for instance, we use an endless india-rubber band to which are cemented by means of india-rubber solution a number of pieces of lead. This band, if put in motion (taking care to keep the pieces of lead on the inside of the loop to prevent them being torn off by the centrifugal force), can be easily drawn out to double its original length.

II. If in place of an elastic band we hang an endless chain over the pulley and put it in motion, we already know that the motion so communicated to the chain has but little tendency to alter the form of the loop in which the chain hangs. In fig. 3, Pl. III., is represented the change produced by the motion in the shape of the loop : the full lines show the form while at rest ; the dotted lines represent the shape when in motion. It will be observed that the change is not great; and this is the case even when the velocity is great. Later on I shall show how the motion produces this change of shape. The motion will of course produce a tension in the chain similar to that produced in the india-rubber band ; but the chain, being much less elastic, is not elongated to any perceptible extent. If we now attempt to alter the form of the loop in which the chain hangs, we shall find that the motion has communicated new properties to the chain: it now resists any effort made to alter its shape ; and after we remove the disturbing force gravitation can only very slowly restore it to its original form. The chain now conducts itself something like a rigid body. When struck near its lowest part, it looks like a bar of lead ; it becomes indented at the point struck, and very slowly loses the impression of the blow.

It has been customary to call this property which the chain has acquired in virtue of its motion by the name of rigidity. There are, however, reasons which have induced me slightly to differ from the ideas generally received on this subject; and it is with the greatest diffidence that I here venture to state

them, as the subject is one of great difficulty, and one of which we have a very limited knowledge. A rigid body is one which is generally supposed to be capable of resisting a certain amount of force without being permanently put out of shape. The force required to put it permanently out of shape may be small; yet it is a perfectly definite amount. Now the chain, when in motion, may have its shape altered by any force, however small, the greater force only making the alteration take place more quickly. This being the case, does not this new property in the chain correspond more to plasticity or viscosity than to rigidity? Does it not conduct itself more like a piece of wax or a mass of treacle or tar than like a piece of lead? Of course I am here using the word rigidity in its extreme sense, a sense in which perhaps no solid can be accurately said to possess it.

Rigidity and viscosity as applied to matter can perhaps scarcely be called different things, but may be more properly called degrees in the same scale, the scale beginning in perfect fluidity, passing through viscosity and plasticity to perfect rigidity; but no substance with which we are acquainted has properties corresponding with either extreme end of the scale.

III. In all the experiments, gravitation acts on the chains while in motion. The next experiments are to illustrate the manner in which gravitation acts on the moving chain and changes its form. When the chain is hanging from the driving-pulley, gravitation is balanced by the tension in the chain, and there is equilibrium; but suppose now that the lower end of the loop is raised to one side of the driving-pulley, as shown in figs. 4 and 5, Pl. III. The chain can easily be put into this position by means of the movable pulley E, fig. 3, Pl. IV. When the movable pulley is removed, and the chain is only supported by the driving-pulley A, then the tension no longer balances the gravitation, and the form of the loop is changed. The chain does not keep its form, and swing as a solid body or as a chain not in motion would do, so as to bring its centre of gravity under the driving-pulley, but the chain in falling to a position of equilibrium passes through a beautiful series of curves. The forms which the chain passes through depend on the direction of motion of the chain relatively to the driving or supporting pulley. If, for instance, the upper part of the chain is moving towards A the driving-pulley, then the chain in falling to its position of equilibrium passes through a series of forms, a few of the intermediate shapes of which are represented in fig. 4, Pl. III., and finally arrives at its position and form of equilibrium similar to that shown in fig. 3, Pl. III. If, however, the direction of motion is the opposite of this, and the upper part of the chain is moving

in a direction *away* from the driving-pulley, then the chain
passes through a series of shapes such as those shown in fig.
5, Pl. III., after which the chain again falls through a series
of forms similar to those shown in fig. 4, Pl. III., before it ar-
rives at its position of equilibrium fig. 3, Pl. III., because the
upper part of the chain in fig. 5, Pl. III., is now approaching
the driving or suspending pulley.

The reason for these different series of forms in the two
cases is very simple. Take the case represented in fig. 4,
Pl. III. Here the links, where they leave the underside of the
pulley, begin to be acted on by gravitation ; but as they are
moving rapidly, gravitation only acts a short time on them ;
their downward motion is therefore very slow. As the links
move further and further from this point, gravitation has had a
longer and longer time to act on them, so that their downward
motion becomes quicker and quicker ; and therefore the links as
they approach the driving-pulley are falling quicker than those
leaving it—the result of which is, the chain in falling passes
through a series of forms such as those represented in fig. 4,
Pl. III. Similar reasoning applies to the case represented in
fig. 5, Pl. III.

IV. We have shown by means of the diagrams figs. 1 and 2,
Pl. III., that the tension just balances the centrifugal force at
all points, and that therefore the chain has no tendency to
change the shape in which it is moving. The next experi-
ments (figs. 1 to 5, Pl. V.) show how this equilibrium may
be destroyed, and also the effect of destroying it. A short
endless chain forming a loop about 20 inches long is hung
over the driving-pulley A, fig. 1, Pl. IV.; and the lower end of
the chain, instead of hanging free, is allowed to rest on the
platform R, fig. 1, Pl. V. When the chain is now put in
motion there is no tension at the lower part of the loop, due to
centrifugal force, because the downward motion of the links
is now destroyed by striking the platform R; and there is but
little tension on the descending side of the loop, and the
tension on the ascending side is due to putting the links in
motion in an upward direction. As the velocity of the chain
increases, the centrifugal force of the part of the chain resting
on the driving-pulley being unbalanced by the centrifugal
force at the lower end, the chain tends to rise off the pulley—
the result of which is, the weight of the chain is gradually
taken off the pulley ; and as this diminishes the friction be-
tween the chain and the pulley, a limit in the velocity of the
chain is soon reached, beyond which it is impossible to drive
the chain, however quickly the pulley is driven. The chain acts
in the opposite way, but with a similar result, to a self-acting

break. In order, therefore, to cause the chain to take up the same velocity as the pulley, it was pressed against it by means of the elastic pulley G, fig. 3, Pl. IV., with the following results.

A. Fig. 1, Pl. V., shows the effect when the chain is pressed by G at o, on the descending side, at the point where it leaves the pulley. There is no alteration in the path of the chain, because the chain after it leaves the pulley is moving in a straight line, and as there is no deviating force there is no centrifugal force, and therefore removing the tension in the chain has no effect on the direction of motion of the links.

B. Fig. 2, Pl. V., shows the effect when the chain is pressed at the point p, a little higher up the pulley on the descending side. In this case, the centrifugal force of the curved part of the chain resting on $o\,p$ on the descending side of the pulley being unbalanced by the tension, the chain rises from the pulley and is shot away from it, as shown—the direction of its motion where it leaves the pulley being a tangent to the pulley at the point p, where it is pressed by the elastic pulley G. Of course the curved part of the chain $p\,q$, resting on the ascending side of the pulley A, also tends to rise, but is kept in its place by the tension produced by putting the chain in motion after being stopped by the platform.

C. Fig. 3, Pl. V., shows the effect of pressing the chain at q on the ascending side of the pulley. The centrifugal force of the curved part of the chain resting on $o\,p\,q$ being unbalanced by the tension, the chain rises up off the pulley in an irregular curve, and only touches the pulley at the point q. When the velocity is sufficient to cause the chain to rise up to such a height that all the slack chain resting on the platform R is taken up, then the conditions become altered. When all the slack chain is taken up, then the centrifugal force produces a tension in the lower part of the chain, and, unless we can keep increasing the velocity of the chain, it can no longer keep its elevated position, because the centrifugal force is now balanced by the tension ; and as the gravitation is now unbalanced, it gradually flattens the curve, till the chain again comes to bear on the top of the pulley, and spreads itself out on the platform.

D. Fig. 4, Pl. V. At the beginning of the previous experiment, as there was no tension in the chain to balance the centrifugal force of the part of the chain resting on the pulley, the centrifugal force overcame the force of gravitation and caused the chain to rise into the air. After all the slack chain resting on the platform had been taken up, and a tension was produced in the chain by the centrifugal force, the centrifugal force of the upper part of the chain was balanced by the tension,

and was no longer free to overcome the gravitation, and the chain began to fall. At this point its fall may be stopped, or it may be caused to rise again, by destroying the tension at the lower part of the chain. This we can do in two ways. We may either cause the chain to strike the platform as nearly as possible at right angles, as shown at fig. 4 ; the motion of the links will thereby be partially destroyed, and the tension at the lower part thereby reduced, and the chain will again rise ; or, if when the chain is meeting the platform at an acute angle, and the upper part of the chain is falling, we turn the platform so as to cause the chain to meet it at a less acute angle, then the chain will again rise ; or we may reverse the experiment. Suppose that while the chain is meeting the platform at right angles, as shown at fig. 4, and is keeping its elevated position, we bend the platform, as shown at R′, so as to cause the chain to meet the platform at an acute angle ; then the chain at once begins to fall in the manner shown by the dotted lines, fig. 4. But so long as it keeps the form shown in full lines, fig. 4, it will keep balanced in its elevated position, for a long time standing on the platform R and only touching the pulley A at the point q,—the reason for this being that, if we partially stop the motion of the links by causing them to strike the platform, or if we partially alter the direction of their motion by causing them to strike the platform at such an angle as partly to change the direction of their motion, then there will be less tension in the lower part of the chain than in the upper, as the tension in the lower part will be only that due to *partially changing* the direction of the motion of the links.

In fig. 3, Pl. V., the chain meets the platform at a very acute angle, and the change in the direction of the motion of the links is almost entirely effected by the tension in the chain, and only to a very small extent by the platform ; there is therefore not sufficient unbalanced centrifugal force at the upper part of the chain to keep it in its elevated position. But the case is different in fig. 4. In this case the platform assists in altering the direction of the motion of the links at the lower part, and the difference between the tensions in the upper and lower parts is sufficient to keep the chain in its elevated position against the force of gravitation.

A chain forming a loop of 4 or 5 feet may easily be kept in the air for any length of time, as it tends to strike the platform nearly at right angles, as shown at fig. 5, Pl. V. If the velocity of the chain is not sufficient to raise all the chain from the platform, then the apparatus under these circumstances is working something like a fountain, in which the driving-

wheel is performing the part of a pump, and the loose chain on the platform the part of the water supply. The different parts of the jet, however, in this case are rigidly connected and all moving at the same velocity, which causes the curve of the chain to differ from that of the water jet.

V. The chains in the previous experiments were, to a certain, degree, rigid. If we give the platform a quick upward motion, the chain rises from the platform like a rigid body, and again falls on it, getting its form but little put out of shape by the treatment—because when it falls on the platform, although it gets slightly flattened, yet it tends to bend so as to cause the links to strike the platform more nearly at right angles, and therefore tends to cause the upper part of the chain to rise again, till the chain is pulled upwards and into such a shape as to reduce the angle at which the links meet the platform, the chain thus again regaining its form of equilibrium. This quasi-rigidity communicated to the chains by the motion is so great, that when thrown off the pulley in rapid motion they run along the ground like wheels. The simplest way of making this experiment is to hang the chain over a pulley which has a flange on one side only, such as that shown at B, fig. 2, Pl. IV. After sufficient velocity has been communicated to the chain, it is easily slipped over the edge of the pulley and dropped on a platform, along which it will run for some distance. The platform ought to be movable, and should be brought as near the lower part of the chain as possible, as the chain gets put out of shape if it falls far. It is also an advantage to put some rough or ridged surface on the platform where the chain drops on it, as it enables the chain to get up a longitudinal motion quickly.

The chains for this experiment may be short, so that when hung over the pulley they form nearly circular loops; or they may be long, so as to form long loops. These long loops do not run along like a solid body, but always keep the longer axis of their figure vertical, as shown at D, fig. 6, Pl. V. As they move along they gradually lose their velocity, and get flattened down and put out of shape. For this experiment we may use light or heavy chains. Chains weighing from 6 to 8 oz. per yard do very well; but any weight of chain may be used. A common watch-guard, for instance, if hung over an 8-inch pulley so as to form a loop 8 inches by about 2 feet, and driven at a great velocity, will, when dropped off the pulley, glide along looking like a polished wire hoop. It is not simply the "rigidity" which enables these chain-wheels to keep their shape and elevated position; the explanation given in experiment IV. D also applies to them.

A variation of this experiment may be made by dropping the chains when in rapid motion on an inclined metal or other polished surface, where they will remain in rapid motion for some time, gradually getting their form flattened by gravitation.

VI. The next experiments are best made with a long chain forming a loop of 10 or 12 feet, figs. 1 to 4, Pl. VI. The chain is put in motion by means of the pulley H shown at fig. 4, Pl. IV.

A. Fig. 1, Pl.VI. If the chain is struck on the descending side of the loop (at, say, the point B), then one part of the wave so formed is carried rapidly with the motion of the chain down the one side of the loop and up the other—so rapidly that the eye cannot follow it. After the wave strikes the pulley on the ascending side at C, it is reflected from it, and travels slowly down the ascending side of the loop C D, gradually becoming smaller and smaller, and dies out before reaching the bottom D. The other part of the wave which was formed when the chain was struck at B, slowly travels upwards against the motion of the chain, till it meets the pulley, when it is reflected, and very rapidly carried round by D to C, where, like the first part of the wave, it is reflected and slowly travels downwards.

B. If we strike the chain on the ascending side at a point a short distance below C, so as to form the wave shown in fig. 1, Pl. VI., then the one part of the wave slowly travels down the chain, while the other part is rapidly carried up to the pulley, where it is reflected, and slowly travels down the chain; and as this reflected wave travels more rapidly than the wave in front of it—on account of the greater tension on the chain, due to gravity, at the wave further up the chain—it overtakes the first wave, and the two form one complete wave, and travel to the bottom together. The successive positions and forms of the waves are represented by the dotted lines in fig. 1.

C. The next experiment illustrates the manner in which gravitation acts on the moving chain. By means of the movable pulleys E and F, fig. 3, Pl. IV., the chain is put into the shape represented in fig. 2, Pl. VI., where H is the driving-pulley and E and F are the two movable pulleys. The chain is then put in rapid motion in this shape, in the direction shown by the arrows. If now the pulleys E and F are suddenly removed so that all the weight of the chain hangs from H, the chain in falling passes through a series of forms somewhat like those represented in the figure. The points most worthy of notice in this experiment are :—1st, the slowness of the descent of the ∽ form ; 2nd, the peculiar balance of the chain :

the chain does not swing as a solid body or chain not in motion would do, so as to bring the centre of gravity under the suspending pulley H ; but the descending side keeps the same position it had before E and F were removed, and the ∽ form alone slowly descends, its shape altering as it falls. The explanation given of experiment III. is also applicable to this case. The peculiar balance of the chain in this experiment reminds one slightly of the balance of the gyroscope. 3rd. At the moment the pulleys E and F are removed a quasi-elasticity may be observed : the chain falls rapidly for a short distance, then stops and rises a little, and then falls again, making two or three vertical oscillations before it settles into a steady descent. The oscillations are probably caused by the unbalanced tension, due to the weight of the chain, on the pulleys E and F, adjusting itself to the new conditions after the pulleys have been removed.

D. In making the next experiment, the chain while at rest is passed round the pulley E, in the manner shown in fig. 3, Pl. VI., so as to form a circular loop near the top on the ascending side of the chain. The chain is then put in motion by means of the driving-pulley H. After the chain has acquired sufficient velocity the pulley E is removed, which is easily done by gently striking the chain in front, just below the pulley E, and at the same time withdrawing the pulley. The loop E being now free, slowly moves down the chain, slightly increasing in size as it descends. Its successive positions are shown by the dotted lines. At first sight it might be thought that the descent of the loop E was entirely caused by its weight. Such, however, is not the case. The downward motion of the circular loop is a true wave motion ; and the velocity of its descent depends on the tension produced by the weight of the lower end of the chain. If we unfold the loop E, as shown at O, we see at once that it is a wave motion, similar to that shown in fig. 2, Pl. VI. This wave motion may still further be illustrated by making the circular loop on the *descending* side of the chain, when the loop will *ascend* the chain. The upward motion on the descending side is not, however, so successful, as the circular loop gets rapidly reduced in size, and only travels a few feet before it is destroyed. This experiment may be varied in another way. We may put a movable pulley into the lower end of the loop at D, so as to enable us to increase the tension in the chain, when we shall find that by increasing the tension we can cause the circular loop to move faster, in accordance with the laws of wave motion.

E. The last experiment with this chain to which we need refer is made in the following manner. The chain is put in

motion in the shape shown in fig. 4, Pl. VI., by means of the movable pulleys E and F. If, while the pulley E is kept steady, a downward motion is given to the pulley F, the chain, instead of pressing harder on the pulley E, rises quite off it, as shown in the dotted lines in the figure ; and if the downward motion of F be continued, the circular part which formerly rested on E rises till it strikes the driving-pulley H. The downward motion of F requires to be quick at first ; but after the chain begins to rise a very slow motion is all that is necessary. The reason for the chain rising from the pulley E is, that the downward motion of the pulley F increases the velocity of the chain at *q r* ; therefore the centrifugal force of the part of the chain resting on E is increased, while the centrifugal force at *s* is not increased. The chain therefore yields at *s* and rises at *r*.

This change in the shape of the chain is perhaps easiest studied in a chain not in motion, but simply fixed at one end, the other end hanging free. If we hang the lower end of the chain over the pulley E, as in fig. 4, Pl. VI., and pull the free end of the chain at F, we get a similar result to what we get when the chain is in rapid motion. But in this case the downward motion of the end of the chain must be much quicker ; and the upward motion of the curved part *s q r* is correspondingly quick—so quick that the eye can scarcely follow it. When we pull the free end of the chain at F we thereby give the chain at *s q r* an upward motion. Suppose that we stop pulling at F after the chain has acquired a motion sufficiently quick to enable it to rise off the pulley E. The curved part will form a regular wave motion, the links at *q* being in rapid upward motion, while those at *r* are gradually losing their motion, the energy of their motion being transmitted by the chain at *q* to put the links in motion at *s*. It is very evident that all the energy lost at the last part of the wave at *r* is not spent in putting the links in motion at the beginning of the wave at *s*, but has to do work against the force of gravitation— because if the energy were simply transferred from *r* to *s*, as in many wave-forms, then the wave *s q r* might travel up any length of chain, and in so doing lift the whole chain the height *s r*, when the energy put into the part *s q r* was only sufficient to raise the part *s q r* to *r*. To enable the wave to travel up the chain we must communicate energy to it while in motion, by keeping up a tension at the lower end of the chain at F. The end of the chain must therefore yield to the tension, and the part *s q r* become less and less as it travels up the chain.

The wave-forms represented in figs. 1, 3 and 4, Pl. VI.,

move very slowly, on account of the motion of the chain; and if we attempt to make the experiments with a chain not in motion, the waves travel so quickly that the eye cannot follow them. If we could reduce the tension in the chain due to its weight, then the waves would travel slowly, even in a chain not in motion. The simplest method of doing this is to lay a length of chain on a horizontal polished surface ; in the absence of a better, a wooden floor will do. One end of the chain is fixed; the other end is then, by means of the hand, caused to take any of these wave-forms. When this is properly done the wave travels along a great length of chain. In making this experiment the most important point to be attended to is not to bring the hand to rest after the wave is started. The hand with the end of the chain must be gradually and steadily withdrawn, so as to keep up a slight tension in the chain ; otherwise the wave will travel but a short distance. In making the experiments in this way, as the tension in the chain is small, the waves move so slowly that they can easily be followed with the eye. Beside the wave-forms mentioned, the chain may be made to take up any others that may be desired ; and the waves may be put in motion either in a horizontal or vertical plane.

VII. All these experiments only illustrate the balance of the centrifugal force and the tension when the motion is confined to one plane. The next experiment is to show that these forces are also in equilibrium when the motion is not confined to any plane, but is constantly changing from one plane to another. In order to illustrate this we can take either the short or long chain used in the previous experiments, and, by means of the movable pulleys, bend the lower part of it, while in motion, into a plane at right angles or at any angle to the plane of motion of the upper part, when we shall find that, though gravitation slowly unbends the chain, the centrifugal force has no tendency to alter its shape. This point, however, may be illustrated in another and better way, if we take a circular disk of paper, or any other flexible material (A, fig. 1, Pl. VII.), and mount it on the apparatus fig. 1, Pl. IV., so that it can be rotated round its centre ; this is easily done by cutting a hole in the centre of the disk and fitting it to the arrangement *c* at the end of the spindle *a*.

Let us first see what the result is if we *first bend* the disk and then put it in motion. The results may be summed up under three heads : 1st, the bent part rotates with the disk ; 2nd, the centrifugal force tends to unbend the disk ; and, 3rd, the elasticity of the disk also tends to restore it to its original shape. If now we *first put the disk in motion* and then bend

it to the form shown at B, fig. 1, Pl. VII., we get quite a dif-
ferent set of results: 1st, the bent part does not rotate with
the disk, but keeps its original position, if the motion of the
disk is kept uniform ; if the speed is either increasing or
decreasing, the bend will go very slowly either forwards or
backwards ; 2nd, there is no centrifugal force tending to
unbend the disk ; and, 3rd, the elasticity of the disk only very
slowly restores it to its original form, as it is resisted by the
rigidity produced by the motion.

Although a disk of paper illustrates these points, yet the
experiment is much improved by loading the circumference of
the disk, as this increases its rigidity without increasing its
statical stiffness. The disks used in the experiments were
about 18 inches in diameter, made of cartridge drawing-paper.
The circumference was loaded with flattened pellets of shot
placed about $\frac{5}{8}$ inch apart, and fixed to the disk by means of a
strong solution of india-rubber. If the weight added to the
disk is such that it just balances the elasticity of the paper,
then the bend remains in the same place, and the disk keeps
the same shape for a very long time, while the disk is rotating
rapidly. The disk may be bent till the circumference touches
the centre (B, fig. 1, Pl. VII.) ; and while the bend keeps its
position the chain of shot passes through many different
planes ; and as the tension just balances the centrifugal force
at all points, the disk has no tendency to alter its shape.

The change in the form of the rotating disk will produce an
alteration in the internal strains in the disk. When the disk
or any other body, such as an ordinary fly-wheel, is rotating,
the centrifugal force is resisted in two ways—partly by radial
tension, and partly by tangential tension ; and the amount
borne in each of these directions will depend on the relative
elasticity of the material in the rim and in the spokes, and on
the manner in which the wheel is constructed. The principal
part of the strain may be borne by either the spokes or the
rim of the wheel, as may be desired. In the paper disk, when
its motion is all in one plane the strain will be borne in both
ways, but when it is bent the strain will be almost entirely
tangential.

Before proceeding further it will be necessary for me to
refer to some of the disturbing forces which I have already
stated to be in action in the moving chain, tending to cause
it to alter its shape. When looking at a chain hung over a
pulley and in rapid motion, fig. 3, Pl. III., the most marked
effect of the motion which we notice is a flattening of the curve
of the chain, just before it begins to turn at the lowest point ;
and after the chain has turned and begun to ascend, there is a

curious reverse curve in the chain, caused by it curving further round than seems necessary, and then requiring to be unbent again. These two alterations in the shape of the loop look as if the motion had conferred a certain degree of rigidity on the chain, which enabled it to resist bending at the entrance to the curve, and also to resist unbending at the other side. There are also alterations in the shape of the loop near the driving-pulley. It will be also noticed that the chain does not now hang in the same position as when at rest; its centre of gravity is evidently a little to one side of its position of rest. This is caused by the manner in which motion is imparted to the chain; the tension on the ascending side of the chain is greater than on the other side, thus causing the centre of gravity to move to the one side.

VIII. The alterations in the curvature of the chain produced by the motion have hitherto been supposed to be due to the friction of the links pivoting on each other, under the great tension produced in the chains by the centrifugal force. That this is not the full explanation, however—though it may, *in part*, explain the flattening of the loop at the entrance to the curve—is easily proved by taking two precisely similar chains, and passing one of them through a flame, so as to dry and oxidize its surface, and oiling the other chain. The only difference between the two chains now is, that there is more friction in the one than in the other when in motion. If we now hang these two chains over the double pulley C, fig. 2, Pl. IV., the two gooves in which are of exactly the same diameter, so as to drive the two chains at the same velocity, we shall thus get what the effect due to friction is. We find that the oiled chain has the reverse curve well marked, while there is no reverse curve in the other chain, the effect of the friction being to make the loop open out and take up a form approaching a circle. Further, if we cause both of the chains to take up the same curvature at the bottom part of their paths, which we can easily do by passing them over a small pulley or a glass rod, we shall then find that the reverse curve is notably least where the friction is greatest.

The alterations in the curvature of the chain when in motion are in all probability almost entirely due to the change of motion which takes place in the links when moving in a path of varying curvature. For instance, when a link is descending the flat part of its path (*a*, fig. 3, Pl. III.), its motion is almost simply one of translation; whereas, when moving round the curved parts, such as *b*, it has a motion of rotation as well as a motion of translation. The result of this is, the links resist the force tending to increase their rate of rotation when passing from

a path of slower to a path of quicker curvature, and after the rotation has been imparted to the links they tend to keep up their rate of rotation, and thus continue the curve much further round than if the chain were not in motion. If the chain were infinitely thin, this would not be the case. The part of the links in the line of tension has no such tendency; it is the parts of the links on the inside and outside of this line which produce the result.

The outside of the curve of the chain, when passing from a straight path to a curved one, is moving too slow, and the inside too quick, to pass round the curve; the chain therefore resists bending; and after the outside of the curve has acquired increased velocity and the inside lost it, the chain cannot move in a straight path till the inside and outside parts have again acquired the same velocity. The different velocities of the outside and the inside of the loop in passing round a curve therefore cause the chain to continue to curve further round than it would do if not in motion. This tendency to cause the chain to continue curving further round would cause the chain to deviate further from its original shape if it were not resisted by the tension produced by gravitation. This varying rotation also explains why the quickest part of the curve is not at the lowest part of the path of the chain, but at a short distance up the ascending side of the chain. The links at the lowest point are still acquiring an increased rate of rotation; and it is not till past this point that they acquire their maximum rate.

IX. These points may be illustrated by a chain in which the links are short and the chain thick, so that the moment of inertia of the links round an axis perpendicular to the plane of motion of the links is as great as possible, and the moment of the tension round this same axis as small as possible. A simple method of constructing such an experimental chain is to fix by means of glue a series of pieces of wood about 1 inch long and of the section shown in fig. 2, Pl. VII., on each side of a strong piece of tape about 1 inch broad. When this chain is hung over the pulley D (fig. 2, Pl. IV.) and put in motion, the reverse curve is very marked (fig. 3, Pl. VII.). So great is the effect of the varying rate of rotation of the links in this chain, that they never take up a steady motion of translation; the links are constantly rotating too quickly the one way or the other, which gives rise to the well-marked series of waves all the way up one side of the loop and down the other.

At A and B (fig. 4, Pl. VII.) I have shown what I suppose to be the manner in which the varying rate of rotation of the

links alters the form of the curves; A shows how the flatten-
ing is produced at the entrance to the curves.   Suppose the
link 1 to be moving in a straight path and just entering on
the curve *c d*, the links 2, 3, &c. have entered the curve.   The
sketch shows the position of the links with regard to the line
of tension.   The links tend to keep their centre line vertical,
and resist moving in the curved line *c d*.   The sketch also
shows that, by doing so, in passing round the curve the ten-
sion on each end of the link is no longer in a straight line,
but acts as a " couple," tending to cause the link to rotate.
Again, B shows the effect of the rotation of the links when
passing from a curved into a straight path; the link 1 is
moving in the curved path *c d*.   The outer parts of the links
are in this case moving quicker than the inner, the result
of which is to throw the centre line of the link inwards,
thus causing the link to move inwards, and also forcing the
link in front inwards, so causing the links to continue to move
in their curved course.   This rotation of the links causes the
tension in the chain to act as a " couple " on the links as
shown, but this time in the opposite direction to the case A,
and tends to destroy the rotation.   But before the rotation is
destroyed, the chain has curved far past its position of rest;
the links have therefore to come back again, and in doing so
have a rotation in the opposite direction given them, which
again causes them to overshoot the mark, from which they
have to be brought back; and thus, by a continued series of
rotations in one direction and then in the other, the well-
marked waves shown in the figure are produced.

We may, by altering the shape of the links, get still more
marked effects—if, for instance, instead of placing the pieces
of wood on both sides of the tape we place the same size of
link all on one side, so that the whole weight of the links shall
be on the one side of the line of tension.   This form of chain,
however, is not so steady in its motion as the other one.   If,
for instance, we place the tape, that is the line of tension, on
the outside of the curve, then the form becomes extremely
unsteady.   If the two sides of the chain do not take exactly
the same curvature, the side with the most curvature tends to
turn round the other one, and the chain, if its construction
admits of it, turns itself inside out, so placing the line of ten-
sion inside the loop.

X. The length of the links has also an influence on the cur-
vature which the chain will assume when in motion.   If we
make two chains of exactly the same size and kind of wire, so
that they shall be exactly alike in every respect except the
length of links, when these two chains are hung over the

pulley C (fig. 2, Pl. IV.) and driven at the same velocity, the loop of the chain with the longest links opens out and tends to take up the circular form, the smaller links keeping near the form the chain had while at rest. In the long-link chain there is no reverse curve, while it is well-marked in the short-linked one.

XI. An elastic cord in rapid motion also tends to assume the circular form, because the internal strains in the quickly curved parts tend to open them out in the same manner as if the cord were at rest. An elastic cord, when in motion, does not take the reverse curve like a chain, because its tendency to do so is resisted by the strain in the material.

XII. In all these experiments gravitation acted on the chains, so that, whatever form we might give them, gravitation soon restored them to their original form of stability. An attempt was therefore made to get quit of the disturbing effects of gravitation, as it was thought that the action of the other forces might be more conveniently studied if its effects were removed. The problem, however, is an extremely diffi-cult one, and has quite baffled all my attempts. Many me-thods suggested themselves for accomplishing this end ; some of them were tried ; but none of them was successful. The next experiment shows the plan which was found most suc-cessful, namely suspension. The chain n (fig. 5, Pl. VII.) is suspended by a number of fine cords $d\,d$ to a circular metal disk e. The disk e rotates freely about a vertical axis, and is placed at a considerable distance above the chain n, so that the cords $d\,d$ may be as long as possible. The disk is also capable of being moved in any direction horizontally. In the experiment this was done by hanging it by means of a long wire. Motion was communicated to the chain by means of the apparatus shown at fig. 5, Pl. IV., the chain being passed round the horizontal driving-pulley i and pressed against it by means of the elastic pulley r.

Let me here briefly refer to the imperfections of this arrange-ment, as the results are modified by these imperfections. First, the effects of gravitation are by no means balanced by this arrangement, as may at once be seen by the position of the cords. This imperfection may, however, be reduced by making the cords long, and by arranging the point of suspen-sion so that it can be moved in every direction, so that the centre of suspension may always be kept vertically over the centre of gravity of the chain. Second, the method of imparting motion to the chain is very imperfect. The chain ought to be perfectly free at all points, whereas this method compels the chain always to pass between the two pulleys. And, third,

the force necessary to keep up the motion is imparted to the chain at one point, which ought not to be, as it produces a tension in the chain which varies in amount at the different parts, being greatest at the part approaching the driving-pulley. These imperfections must always be borne in mind when experimenting with this apparatus.

To set the chain in motion, it is first put round the pulley i and then tightened by holding it out by means of one of the movable pulleys, in the form shown at fig. 6, Pl. VII. After the apparatus is set in motion, the movable pulley may be removed, as the centrifugal force tightens the chain. When in motion, we shall find that we can, by means of the movable pulleys, mould the chain into a variety of curves, and that it will retain for some time whatever shape we give it. We may also notice that it resists our efforts to alter its form, and that, after we have succeeded in altering its form, the motion has but little tendency to change the shape of the curves.

These are, however, only the general results which strike one at first. On more careful experimenting, the imperfections of the apparatus and the effect of the varying rate of rotation of the links become very evident. If we continue the experiment for any length of time, we shall find that the chain slowly changes its shape when put into most forms, and that it is only stable in a very few forms. If, for instance, the centre of suspension is kept over the centre of gravity of the chain, it will keep the circular form, or it will keep the form shown at fig. 7, Pl. VII., though it will not keep the form shown at fig. 6, Pl. VII., because the chain tends to continue the bend at a on account of the rotation of the links. It keeps the form shown at fig. 7, Pl. VII., because the links are prevented from curving any further at a by the tension produced in the chain by the pulley in keeping up its motion. The chain does not tend to curve inwards at b, partly on account of the pressure of the pulleys i and r on the chain tending to cause the links to leave the pulley i at the point of contact, and partly because there is less tension in the chain at b than at any other part, as the tension produced by keeping up the motion is least at this part. It is well to remember that the half of the chain next the pulley is so influenced by the pulleys and difference in tension, that it is difficult to draw conclusions from the action of the chain at this part. We may show the imperfections of the apparatus in another way. If, for instance, the line drawn vertically through the point of suspension falls to one side of the centre of gravity of the chain, then the chain will keep an oblong shape; but if the point of suspension is gradually brought

over the centre of gravity of the chain, then the form of the chain changes and approaches that of a circle, and the chain may again be made to take its original oblong form by moving the point of suspension to its original position. The circular form which the chain takes up when the centre of suspension is kept over the centre of gravity is due to friction, length of links, and other causes.

The effect of the varying rate of rotation of the links on their own axis is also well marked, especially if we give the chain a quick curve at any point. The effect, however, is very different from what we get when the chain is hung over the driving-pulley. When the chain is hung over the driving-pulley, there is a tension in the chain due to gravitation; this tension, coupled with the rotation of the links, gives rise to the wave-form which we saw certain forms of chain took up when in motion, fig. 3, Pl. VII. The tension due to centrifugal force has no such effect. When, therefore, in this experiment the chain is suspended and the tension due to gravitation removed, there is but little tension preventing the chain from continuing to curve further in the same direction. The chain therefore goes on bending further and further round until it comes into collision with the part of the chain moving in the opposite direction, and stops the motion, even though the chain at this point is bending out of the way on account of the resistance offered by the links at this part to rotation on their axis. This effect is best shown by using a chain specially prepared for showing the effect of the varying rate of rotation of the links, such as the one used in experiment IX.

While these experiments illustrate the laws of motion, they also in a somewhat rough way illustrate certain early speculations on the structure of matter which have recently been revived and founded on a scientific basis. In these experiments, as in the well-known vortex-ring, motion confers certain properties which we are in the habit of associating with the solid condition of matter. This, however, is a subject alike beyond my province and my powers; I must therefore take leave of it, and conclude by expressing a hope that the experiments may be useful to those engaged in the higher and more difficult investigation of the subject.

Darroch, Falkirk.

XV. *Short Reports from the Chemical Laboratory of Trinity Col-*
lege, Dublin. By J. EMERSON-REYNOLDS, *M.D., M.R.I.A.,*
Professor of Chemistry, University of Dublin[*].

No. 4.—*On a New Form of Measuring-Apparatus for a*
Laboratory-Spectroscope [†].

THE measuring-apparatus for a laboratory-spectroscope
which I have been asked to describe, was fitted about a
year ago to an instrument in common use in the College La-
boratory, and has afforded very satisfactory results. My chief
aim in planning the arrangement was, to facilitate the measure-
ment and identification of spectral lines and the mapping of
spectra under circumstances admitting of little general illu-
mination.

The spectroscope to which the apparatus is fitted has two
fixed flint-glass prisms, the refracting angle of each being 60°.
This instrument is shown in the annexed engraving. When
in use the prisms are covered by a brass cap provided with
openings for the collimating- and observing-telescopes. The
movable arm D that supports the observing-telescope also
carries a vernier which is moved with the telescope over a
graduated arc; and in this usual way the relative positions of
the several lines of a given spectrum can be determined. The
angular distance traversed in passing from the extreme red
to extreme violet is necessarily small, owing to the low dis-
persive power of the instrument; but this, I need scarcely say,
is an advantage rather than the reverse in a spectroscope
which is commonly employed as an aid in ordinary qualitative
analysis.

The graduations of the arc are unavoidably close and diffi-
cult to read in a feeble light; consequently the eyes of the
observer become speedily tired and unfitted for the examination
of faint spectra. Nevertheless measurements made with the
graduated arc and vernier are, in my experience, more trust-
worthy and satisfactory than those obtained with even the best
photographed scale that I have had the opportunity of working
with. Desiring, then, to retain the method of direct angular
measurement, I sought to multiply the motion in such a manner
as to obtain wide readings on a convenient scale. After many
trials in different directions, the form of apparatus which I
shall now describe was finally adopted.

* From the Scientific Proceedings of the Royal Dublin Sociesy; com-
municated by the Author.
† For Report No. 1, "On Glucinum: its Atomic Weight and Specific
Heat," see Phil. Mag. [V.] vol. iii. p. 38; for No. 2, On a New Mineral
Borate, and for No. 3, On an Analysis of Lievrite by Mr. Early's Me-
thod, see Phil. Mag. [V.] vol. iii. pp. 284 and 287.

Description of the Apparatus.—The annexed woodcut, which is taken from a photograph, represents the whole apparatus.
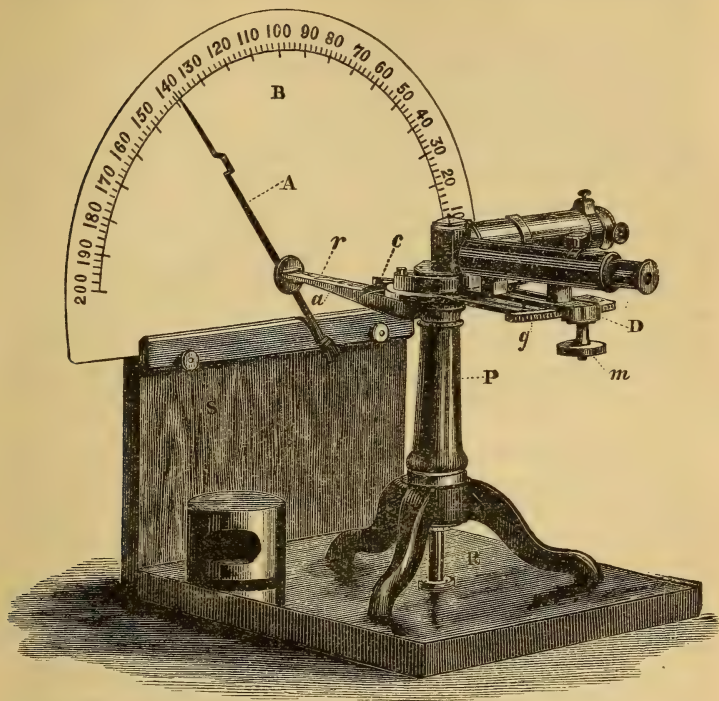
The index A, attached to the spectroscope, moves in front of a graduated plate of opal glass, the latter being supported, in the manner shown, by the stand S*, to which the spectroscope is also screwed by means of the rod R. The index is attached to a milled head which moves stiffly on a stout steel rod *r* ; the latter can revolve in little bearings supported by the projecting arm of "angle-brass," *a*, the other end of the rod being let into a hole drilled in the head of the pillar, P, of the instrument. On the rod just mentioned, and immediately beneath *c*, a small toothed wheel is securely keyed. The diameter of this wheel is about one centimetre, and the teeth upon it are fine and well cut. *c* is a stout metallic strip, five centimetres long, whose lower edge is serrated so as to correspond accurately

* The stand is of stout walnut-wood. A rebate of the thickness of the glass plate is cut to the depth of three centimetres from the vertical piece of the stand. The straight edge of the plate is laid in the groove and is there secured, in part by a pin passing from behind through a hole drilled in the glass, and in part by a wooden slip screwed on in front.

with the teeth of the wheel on the rod r, and to act upon the teeth directly so as to cause the rod carrying the index, A, to rotate easily. This strip is bent to a curve whose radius is equal to the distance from the axis of the pillar, P, of the instrument to the middle of the toothed wheel. The strip is attached to a stout arm ; and this is in turn screwed to the slightly projecting end of the heavy plate, D, which carries, and of course moves with, the observing-telescope, the motion being communicated to the latter by turning the milled head m. As the observing-telescope moves over the graduated arc g, the index A moves in front of the graduated plate B, but in the opposite direction; for the motion of D is communicated to the rod r by means of the serrated slip c. When the fittings are well made, the movement of the index A is steady and corresponds in both directions with those of D. By the simple means described, a very slight motion of the observing-telescope produces a comparatively considerable displacement of the index A.

In my instrument, the telescope and the index move in opposite directions. Any objection on this score can be removed ; for it is only necessary to point out that the motions may be made to coincide in direction by placing c under instead of over the toothed wheel.

Graduation of the Glass Plate.—It is very desirable that the graduations on the plate and on the arc of the instrument should agree ; the best mode of securing this is to graduate the plate with the aid of the arc. For this purpose the telescope is moved into such a position that the rays less refrangible than the red potassium-line shall occupy the field of view ; the zero of the vernier is then made to coincide with the nearest convenient degree marked on the arc. The rod r is then firmly grasped and the index A brought down to a horizontal position, and a fine dot made on the plate under the point by means of a pen dipped in " black japan." This point is taken as the zero of the scale. Each half-degree is marked off in a similar manner until the semicircle is graduated. The two scales are again compared at different points, and the opal-glass plate removed ; each large division, corresponding to half a degree, is then subdivided into 10 * equal parts. Finally the semicircle is numbered from zero up to 200 : each division of the scale therefore corresponds to $3'$ of the arc g. In my spectroscope the angular motion of the observing-telescope is magnified 25 times, and the width of each division of the glass scale is $2\frac{1}{2}$ millimetres, so that the readings are easily made in a feeble light without straining the eyes of the observer.

* In the woodcut only five subdivisions are shown.

Reading off Positions of Spectral Lines.—In commencing an observation it is always desirable to see that the point of the index A stands at the zero of the glass scale when the telescope is in the corresponding position on its scale. Any adjustment of the index that may be necessary is easily made in the way already described—namely, by firmly holding the rod *r* and turning the milled head which carries the index to the desired extent. The actual reading of the position of a line to which the point of the fine needle in the eye-piece is brought is then made from the glass scale.

An exceedingly feeble light suffices to enable the operator to read the wide divisions on the white scale ; but in observing very faint lines I do not read by reflected light, but faintly illuminate the scale by means of a very small gas-jet or lamp placed *behind* it. Sufficient light is transmitted by the opal glass to enable the readings to be easily and quickly made, while the eye of the operator is retained in a sensitive condition for feeble rays. Moreover, in reading, it is not necessary to move the head away from the eyepiece of the instrument.

I have tried with success a mode of determining small differences with this apparatus, which could doubtless be applied with advantage in mapping spectra with instruments of high dispersive power.

The glass plate B was removed from its stand and the index from the rod *r* ; I then attached to the latter a cork carrying a small mirror placed at a suitable angle. A spot of light was reflected from this mirror and made to fall on a screen placed several metres away. The relative distances between the members of groups of closely ruled lines (those of the nitrogen spectrum) were then easily determined in this manner, as the actual motion of the needle from point to point was greatly magnified.

The relative positions and widths of the lines seen with the instrument are easily laid down on a millimetric scale. I have had a number of 200-m.m. scales printed on narrow slips of paper ; and the graduations are lithographed on a band of six equidistant lines, which thus serve for marking off intensities, according to Bunsen's graphic method. One millimetre corresponds to one unit of the scale on the opal-glass screen, and consequently to three minutes as read off with the vernier on the graduated arc of the instrument. Differences corresponding to 1′ can therefore be easily estimated and represented on the millimetric scale.

But one other practical point need be mentioned. I find it exceedingly convenient to mark off on the opal-glass scale the

positions of the more important lines of the elements whose spectra are easily obtained with the aid of the Bunsen flame. The symbol of the element to which a particular line or band belongs is legibly written under the particular point of the scale, and connected by a line with the point in question. Identification of the bright lines observed in the spectrum of an unknown compound is thus greatly facilitated.

I may be permitted to add that the measuring-apparatus described has been fitted to the spectroscope used in the College Laboratory by Messrs. Yeates and Son of this city, whom I have to thank for the care and skill with which they carried out the details of construction.

XVI. *On a Method of measuring the Absolute Thermal Conductivity of Crystals and other rare Substances.*—Part I. *By* OLIVER J. LODGE, *D.Sc.*[*]

1. WHEN only a small portion of a substance is obtainable on which to experiment, the measure of conductivity by any dia-calorimetric method becomes difficult; and accordingly observers have contented themselves, in the case of the rarer crystalline bodies, with comparing their conductivities in different directions by Sénarmont's or some similar method. If the substance is sufficiently plentiful to be obtained in slabs (like rocks), then some modification of Fourier's " thermomètre de contact " will give its conductivity, though there are many objections to the use of this instrument.

But there is another method of Fourier's, applicable only to long rods, put in practice by Biot, Despretz, Forbes, and recently by Wiedemann and Franz (commonly known as Forbes's method), which it seems possible to modify so as to make it applicable to short rods or even slices[†]. This well-known method consists in observing the permanent curve of temperature along a cylindrical rod of the given material, one end of which is heated

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[*] Communicated by the Physical Society.

[†] The method occurred to me when thinking how best to measure the conductivity of tourmaline in opposite directions along the axis, a subject which I was considering in conjunction with Mr. S. P. Thompson of Bristol; for we had reason to think that tourmaline and all other pyro-electric crystals must necessarily possess a unilateral conductivity along their axis both for heat and electricity; and this supposition has been partially confirmed, in the case of heat, by some preliminary experiments of Mr. Thompson's last summer on a very small crystal. No further confirmation or modification of the experiment, however, has yet been possible, owing to the scarcity of the crystal and the difficulty of obtaining a large slice; but this difficulty has now been removed by the kindness of Professor Nevil Story Maskelyne.

and the rest exposed to the atmosphere. Let s be the area of cross section (which need not be circular) and P the perimeter of the rod, the latter being defined as the length of a string wrapped once round the rod if the actual perimeter is any *re-entrant* curve. The condition to be expressed is that the total gain of heat of any element of the rod by its anterior, posterior, and exterior surfaces is equal to nothing. Taking t as the excess of temperature over that of the air of an element in a position x along the rod, we have the quantity of heat

$-ks\dfrac{dt}{dx}$ entering at its anterior or hotter surface in unit time,

$+ks\left(\dfrac{dt}{dx}+\dfrac{d^2t}{dx^2}dx\right)$ at its posterior surface,

$-\mathrm{P}ht\,dx$ at the surface exposed to the air;

k being the conductivity of the rod, and h the radiation-coefficient of its surface, *i. e.* the quantity of heat lost in unit time by unit surface when it is one degree hotter than the air.

Putting the sum of these quantities equal to zero, we have

$$\frac{d^2t}{dx^2}=\frac{\mathrm{P}h}{sk}\,t=p^2t\text{ say,}$$

an equation whose complete integral is

$$t=\mathrm{C}_1e^{px}+\mathrm{C}_2e^{-px},$$

or, as I shall prefer to write it,

$$t=\mathrm{A}\cosh px-\mathrm{B}\sinh px.$$

The constants A and B are determined in terms of p as soon as one knows the temperatures of any two points of the rod; and the temperature of a third point will determine p, whence, if h be separately found, k is known. Thus, suppose we know t_0 the temperature of the origin, and t_x the temperature of a point at a distance x from it, and also $t_{\frac{1}{2}}$ the temperature half-way between these points; then

$$\mathrm{A}=t_0,$$
$$\mathrm{B}=t_0\coth px-t_x\operatorname{cosech}px,$$
$$p=\frac{1}{x}\cosh^{-1}\frac{t_0+t_x}{2t_{\frac{1}{2}}}.$$

2. Now suppose the rod to be cut in half and a slice of crystal or any substance interposed: the curve of temperature will have a discontinuity at the junctions; but if the curve along each rod is observed, it may be possible to calculate it for the crystal. The method which I propose, then, is to cut a cylindrical piece of the substance to be examined, of length z, with

flat faces, and to squeeze it between two copper or iron rods (or any other metal whose conductivity is well known) of exactly the same cross section as itself both in shape and size, putting a pad of a few thicknesses of tinfoil between the surfaces, so as to make better contact, and then to observe the curve of temperature down each rod when one end of one is heated and the further end of the other is cooled, the whole having been left long enough to attain a permanent state.

Conduction through a cylinder inserted between a pair of metal rods.

3. Let the cylinder be of length z,
and conductivity χ,
and let its surface have the radiation-coefficient h'.
Let the packing on each side be of thickness y,
and conductivity κ,
and let it be so thin that radiation from its edge is negligible.

Fig. 1.
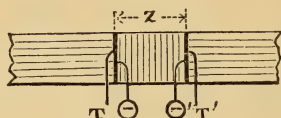
Also let us take the atmospheric temperature as an artificial zero; so that by "temperature" we shall always mean *excess of temperature* above that of the air.

And let T and Θ be the temperatures of metal and cylinder on each side of first packing (see fig. 1),
T' and Θ' ditto on each side of second packing.
Then the quantity of heat which leaves the first rod traverses the first packing and enters the cylinder, which is expressed analytically thus (k being the conductivity of the metal),

$$k\frac{d\mathrm{T}}{dx} = \kappa\frac{\Theta - \mathrm{T}}{y} = \chi\frac{d\Theta}{dx}; \quad . \quad . \quad . \quad (1)$$

and similarly for the quantity which crosses the second packing,

$$k\frac{d\mathrm{T}'}{dx} = \kappa\frac{\mathrm{T}' - \Theta'}{y} = \chi\frac{d\Theta'}{dx}, - \quad . \quad . \quad . \quad (2)$$

four equations from which the unknown quantities, $\frac{\kappa}{y}$, Θ, and Θ' can be eliminated, and χ be found.

Now the curve of temperature down the cylinder is, from § 1,

$$\theta = \Theta \cosh qx - (\Theta \coth qz - \Theta' \operatorname{cosech} qz) \sinh qx, . \ (3)$$

where

$$q^2 = \frac{\mathrm{P}h'}{s\chi}, \qquad \cdots \cdots \cdots \quad (4)$$

hence

$$\frac{d\Theta}{dx} = q\,(\Theta'\,\mathrm{cosech}\,qz - \Theta\,\coth qz)$$

and

$$\frac{d\Theta'}{dx} = q\,(\Theta'\,\coth qz - \Theta\,\mathrm{cosech}\,bz).$$

Substituting these values in equations (1) and (2) and eliminating, we get from the first and second of each set

$$\Theta\frac{d\mathrm{T}'}{dx} + \Theta'\frac{d\mathrm{T}}{dx} = \mathrm{T}\frac{d\mathrm{T}'}{dx} + \mathrm{T}'\frac{d\mathrm{T}}{dx} = \frac{d}{dx}(\mathrm{TT}')\,;$$

also from the first and third of each set,

$$\Theta = \frac{k}{\chi q}\left(\frac{d\mathrm{T}'}{dx}\,\mathrm{cosech}\,qz - \frac{d\mathrm{T}}{dx}\,\coth qz\right),$$

$$\Theta' = \frac{k}{\chi q}\left(\frac{d\mathrm{T}'}{dx}\,\coth qz - \frac{d\mathrm{T}}{dx}\,\mathrm{cosech}\,qz\right).$$

Therefore, combining all these,

$$\frac{\chi q}{k}\sinh qz = \frac{\left(\dfrac{d\mathrm{T}'}{dx}\right)^2 - \left(\dfrac{d\mathrm{T}}{dx}\right)^2}{\dfrac{d}{dx}(\mathrm{TT}')}. \qquad \cdots \quad (5)$$

Hence χ is determined in terms of q, which itself contains it together with the radiation-coefficient h' (which must be supposed known). We may write the last equation thus, by (4),

$$\frac{\sinh qz}{q} = \frac{sk}{\mathrm{P}h'}\cdot\frac{\left(\dfrac{d\mathrm{T}'}{dx}\right)^2 - \left(\dfrac{d\mathrm{T}}{dx}\right)^2}{\mathrm{T}\dfrac{d\mathrm{T}'}{dx} + \mathrm{T}'\dfrac{d\mathrm{T}}{dx}}, \qquad \cdots \quad (6)$$

which shows that this method is not satisfactory for determining q when the product qz is very small.

4. Now there are three special cases, depending on the value of h':—

(1) When the crystal has its natural surface, and the value of h' is determined by special experiment on its rate of cooling. In this case the above equation remains as written, and may be treated, as qz will generally be less than unity, by expanding the left-hand member,

$$z\left(1 + \frac{q^2 z^2}{\underline{|3}} + \frac{q^4 z^4}{\underline{|5}} + \ldots\right),$$

and then solving for q by successive approximations.

(2) When the crystal and rods are covered over with a coat of varnish (as Brunswick black), so that h is the same for all. In this case the coefficient of the right-hand member becomes simply $\dfrac{1}{p^2}$, but the treatment required is the same as in the first case.

(3) When the crystal is surrounded with cotton-wool or felt, or in some other way has its exterior surface made adiabatic, so that $h' = 0$. In this case the left-hand member of the above equation equals z, and the right-hand becomes indeterminate, so that a fresh investigation is necessary.

Case when radiation from the exterior surface of the cylinder is prevented.

5. Heat will now flow through the cylinder as through part of an infinite wall, and $\dfrac{d\theta}{dx}$ becomes simply $\dfrac{\Theta' - \Theta}{z}$; hence the two sets of equations (1) and (2) are now all equal to one another, and they reduce to

$$k\frac{d\mathrm{T}}{dx} = \kappa\frac{\Theta - \mathrm{T}}{y} = \chi\frac{\Theta' - \Theta}{z} = \kappa\frac{\mathrm{T}' - \Theta'}{y}. \quad . \quad . \quad (7)$$

But as these are only three equations between four unknown quantities, some further observation is necessary before we can determine χ. We may either omit the crystal altogether, or, what is probably better, replace it by a piece of the same metal as the rods are made of, and repeat the temperature-determinations, the packing being kept exactly the same as before. Denoting the temperatures in this case by small letters, the equations will now be

$$k\frac{dt}{dx} = \kappa\left(\frac{\theta - t}{y}\right) = k\frac{\theta' - \theta}{z'} = \kappa\left(\frac{t' - \theta'}{y}\right), \quad . \quad . \quad (8)$$

where z' is the thickness of the bit of metal. By these three equations $\dfrac{\kappa}{y}$ is determined; and its value may then be substituted in the former set.

Eliminating Θ and Θ' from the former set (7), we have

$$\left(\frac{zk}{\chi} + \frac{2yk}{\kappa}\right)\frac{d\mathrm{T}}{dx} = \mathrm{T}' - \mathrm{T}; \quad . \quad . \quad . \quad (9)$$

similarly from (8) we get

$$\left(\frac{z'k}{k} + \frac{2yk}{\kappa}\right)\frac{dt}{dx} = t' - t,$$

whence, getting rid of $\dfrac{\kappa}{y'}$, we have

$$z\frac{k}{\chi} = z' + \frac{T'-T}{\dfrac{dT}{dx}} - \frac{t'-t}{\dfrac{dt}{dx}}; \quad . \quad . \quad . \quad (10)$$

so χ is determined by the two observations.

6. Although this method requires two experiments while the other (§ 3) required only one, yet it will probably be more useful than the other, as it is applicable to very thin slices of crystals—in fact the thinner the better,—whereas the other one applies more to substances in the form of a short rod ; for it fails when z becomes very small. This failure of the § 3 method is due to the fact that it depends entirely on radiation going on from the cylinder at the same intrinsic rate as from the rods, and on some appreciable quantity of heat being lost in this way during its passage through the cylinder ; hence of course a certain *length* of the cylinder is essential.

Observation of the Curves of Temperature.

7. We have seen (§ 1) that, to determine the curves of temperature, it is necessary to know the actual temperature of two, or perhaps one should rather say three, points on each rod. Each rod then should have three holes bored to receive the bulb of a thermometer, one near each end, at a distance l from one another, and another in the middle halfway between the other two. Let the temperatures which these thermometers indicate above that of the air at the time be denoted by T_0, $T_{\frac{1}{2}}$, T_1, T_2, $T_{\frac{5}{2}}$, T_3, their position being shown in the figure.

Fig. 2.
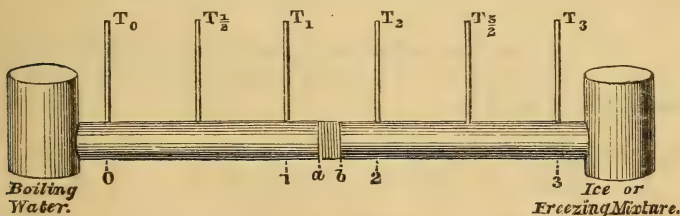
Denote the distances thus :—

$$0\,a = l_0 = b\,3,$$
$$1\,a = l_1 = b\,2,$$
$$0\,1 = l = 2\,3 = l_0 - l_1 ;$$

then measuring x from 0 for the first rod, we have as the curve

of temperature down it, by § 1,

$$t = A \cosh px - B \sinh px;$$

that is,

$$t = T_0 \cosh px - (T_0 \coth pl - T_1 \operatorname{cosech} pl) \sinh px; \quad . \quad (11)$$

similarly down the second rod, reckoning x from b, the curve is

$$t' = A' \cosh px - B' \sinh px,$$

where

$$A' = \frac{T_2 \sinh pl_0 - T_3 \sinh pl_1}{\sinh pl},$$

$$\left. \right\} \quad . \quad . \quad . \quad (12)$$

and

$$B' = \frac{T_2 \cosh pl_0 - T_3 \cosh pl_1}{\sinh pl}.$$

We can now at once express the values of the "known quantities" which occur in equations (1) and (2), and in the right-hand members of equations (5) and (6), viz.

$$T, \quad T', \quad \frac{dT}{dx}, \text{ and } \frac{dT'}{dx}.$$

Thus

$$T = \frac{T_1 \sinh pl_0 - T_0 \sinh pl_1}{\sinh pl},$$

$$\frac{dT}{dx} = \left(\frac{dt}{dx}\right)_{x=l} = p \frac{T_1 \cosh pl_0 - T_0 \cosh pl_1}{\sinh pl},$$

$$T' = A' = \frac{T_2 \sinh pl_0 - T_3 \sinh pl_1}{\sinh pl},$$

$$\frac{dT'}{dx} = \left(\frac{dt'}{dx}\right)_{x=0} = -pB' = p \frac{T_3 \cosh pl_1 - T_2 \cosh pl_0}{\sinh pl}.$$

$$\left. \right\} \quad . \quad (13)$$

The symmetry of these expressions is visible in the following, where for shortness sh_0 is written instead of $\sinh pl_0$, ch_1 for $\cosh pl_1$, and so on:—

$$T : \frac{dT}{pdx} : 1 = \begin{vmatrix} sh_0 & sh_1 \\ T_0 & T_1 \end{vmatrix} : \begin{vmatrix} ch_0 & ch_1 \\ T_0 & T_1 \end{vmatrix} : \begin{vmatrix} sh_0 & sh_1 \\ ch_0 & ch_1 \end{vmatrix} \quad (14)$$

8. We can now write down the value of the right-hand member of equation (5) thus,

$$\frac{\left(\frac{dT'}{dx}\right)^2 - \left(\frac{dT}{dx}\right)^2}{T \frac{dT'}{dx} + T' \frac{dT}{dx}} = \frac{p^2 \{(T_3 ch_1 - T_2 ch_0)^2 - (T_1 ch_0 - T_0 ch_1)^2\}}{p(T_1 T_3 - T_0 T_2) \sinh pl}; \quad (15)$$

so (6) becomes

$$\frac{\sinh qz}{q} = \frac{h}{h'} \cdot \frac{\{(T_0 + T_3)\cosh pl_1 - (T_1 + T_2)\cosh pl_0\} \begin{bmatrix} -(T_1-T_2)\cosh pl_0\} \\ \{(T_0-T_3)\cosh pl_1 \end{bmatrix}}{(T_0 T_2 - T_1 T_3)\sinh pl}, (16)$$

which is a form convenient for calculation.

9. So also for the second method (§ 5), we can write down the value of the quantities occurring in the right hand of (10),

$$\frac{T' - T}{\dfrac{dT}{dx}} = \frac{(T_1 - T_2)\sinh pl_0 - (T_0 - T_3)\sinh pl_1}{p(T_0 \cosh pl_1 - T_1 \cosh pl_0)}; \quad (17)$$

and similarly for the small t's, of which a set t_0, t_1, t_2, t_3 have been observed. In this case there is no loss of heat in passing through the crystal; so we ought to have

$$\frac{dT}{dx} = \frac{dT'}{dx},$$

which gives the condition

$$\frac{T_1 + T_2}{T_0 + T_3} = \frac{\cosh pl_1}{\cosh pl_0}; \quad \ldots \ldots (18)$$

and unless this condition is satisfied there is some error in the experiment, and it is useless to proceed.

I have to express my thanks to my brother, Mr. Alfred Lodge, of St. John's College, Oxford, for several suggestions in the writing out of the above and for some improvements in the notation.

In the second part of this communication some practical details will be given, together with the results of some trials of the method now going to be made.

University College, London.

XVII. *Application of the Kinetic Theory of Gases to Gravitation. By* S. Tolver Preston[*].

No. III.[†]

1. IN the last Number of the Philosophical Magazine is a short paper by Mr. James Croll on Le Sage's Theory of Gravitation, in which he alludes to a difficulty that has

presented itself to him after reading my two former papers on this subject. As any theory that makes a pretention to truth ought to court every criticism, I am glad to notice here the difficulty alluded to, at the same time availing myself of the opportunity to touch upon certain other points that would appear to want a little further elucidation.

2. The point in question is, that, since gravity is proportional to mass, it is admittedly necessary to assume that the total volume of free space in a substance must be great compared with the total volume of matter contained in the molecules of the substance (in order that the medium producing gravity may be able to penetrate the substance and act upon the molecules in the interior). Mr. Croll finds a difficulty in reconciling this assumption with some deductions regarding molecules by Sir William Thomson, in a paper published in 'Nature,' vol. i. (p. 551). Now I think it may be shown clearly here (and that this will also be apparent to Mr. Croll on referring more minutely to the wording of the above paper) that this paper was not intended strictly to give molecular *dimensions*, but rather molecular *distances* (from centre to centre), or number of molecules in unit of volume. It is true that an estimate of molecular dimensions is given on the special assumption that the radius of a gaseous molecule is equal to "half the average shortest distance reached in a vast number of collisions." Whether this is the actual radius, therefore, depends evidently on whether the two molecules come into *contact* at collision or not. This might not be; and if not, the radius might be smaller. Thus it is at least conceivable that a layer of a medium may exist between two vibrating approximated molecules, much as a drop of water floats on a film of air. I do not wish to insist upon this comparison; but no one will, I think, consider that it is *necessary* that molecules should come into contact; and if not, it is impossible to measure their dimensions, but only their sphere of activity. This, therefore, would remove the difficulty; but I do not wish to hold necessarily to this explanation, as it appears to me that there are some grounds for supposing that molecules do come into contact.

3. The explanation I have to bring forward is of another character. *The interstices are in the molecules themselves.* This explanation was also, I believe, suggested by Le Sage himself. The old notion of a molecule being a hard spherical mass certainly appears rather crude. In view of the numbers of different capacities for vibration possessed by a molecule, as proved by the spectroscope, it appears a necessary deduction that a molecule must be of a complex structure. Interstices would make it complex. In ordinary architecture we

do not observe a solid block structure, if I may so express it, but a more or less *open* structure as consistent with lightness combined with elasticity. So molecular architecture (as size is only *relative*, and *principles* apply everywhere the same) may be of an *open* structure, as consistent with elasticity. This open structure, involving various separated parts, would give the molecule the faculty of taking up various vibrations, as it is known to be capable of doing.

4. Admitting, therefore, molecules to possess an open structure, the passage of the gravific medium through the molecules of matter might be compared (merely for a simile) to the passage of a stream of air through a scaffolding, the air passing in great part through, but exercising a gentle pressure against the solid parts of the scaffolding. So, in analogy, with the gravific medium ; or by this open structure of molecules we have all the conditions for the pressure termed " gravity," together with the permeability essential to make gravity proportional to mass. [We make no postulate as to the *form* of open structure.]

5. One point may be noticed here in connexion with the inference that the molecules of solids are in *contact*. The old postulate of perfectly *rigid* molecules put a difficulty in the way of assuming that the molecules of a solid are in contact, because the " elasticity " (or compressibility within certain limits) of a solid could scarcely be reconciled with this postulate of perfectly *rigid* molecules. The dynamical theory of molecules put forward by Sir William Thomson, which explains the elasticity of a molecule by a simple motion of the matter forming it, enables us to explain the elasticity of a solid (with molecules in actual contact) *by the elasticity of the molecules themselves.* By this theory also the *open* structure we have suggested becomes a natural consequence.

6. That matter does possess an open structure, due to some cause, appears to be sufficiently proved by independent facts. How otherwise could waves of light and the magnetic disturbance pass so freely through matter? It appears natural to assume that the molecules of a solid are in contact, on account of the resistance they oppose to displacement in all directions. If so, it would appear necessary to look for the interstices in the molecules themselves ; and we think we have shown that this conclusion is not merely warranted by the case of gravity, but that it is in itself rather probable on independent grounds.

7. It may be observed that, by means of interstices in the molecules themselves, a mass may possess any degree of openness and yet be practically closed—*i. e.* closed to the penetration of all ordinary matter, such as the air, liquids, &c.,—as

evidently one molecule cannot readily penetrate into the interstices of another. On the other hand, the minute particles of the gravific medium pass through them with perfect freedom; and though these interstices are so small, they are on the other hand so numerous (on account of the number of the molecules) that their total sum may represent a relatively very large vacant space. Under these conditions matter may be practically solid or continuous, because impenetrable by the finest portions (molecules) of other matter, and yet possess any desired degree of openness.

8. We would add a few remarks here in regard to the logical necessity of seeking a cause for gravitation. To do so is, as it seems to us, simply to look for an explanation of a natural phenomenon consistent with reason. One sometimes comes across the remark that the effect is an *ultimate* one, incapable of explanation. But then the physical investigator does not readily surrender the right of using his reason; or we really have no power to assume that physical effects are brought about in a way incapable of appreciation by the reason. The most eminent minds have admittedly been in favour of an explanation. This was so (as is known) with Newton and Faraday. Count Rumford says, " Nobody surely in his sober senses has ever pretended to understand the mechanism of gravitation." Physical effects are generally admitted to be fundamentally effects of *motion* (however diverse they may be). The *one* fundamental cause, therefore, to get an insight into in physical science, is the cause of the development of *motion*. If we made an exception to this in any case (or assumed the motion developed was an *ultimate* fact incapable of explanation), then this would be pursuing a course which, if carried out in its entirety would leave *nothing* to be explained at all; for it should be observed that the development of *motion* is in principle the *one* physical effect that requires explanation (from the fact that *all* physical effects are effects of *motion*). This inference surely deserves a mature realization. In the case of gravity we observe a *motion* of approach developed in two masses. Here, therefore, we have an instance of the *one* fundamental fact for which in principle an explanation is required. We require an insight into the cause of the development of this *motion* in the two masses. We want something more than merely to observe the *fact* of the motion; we want (among other things) to understand why the energy of the motion developed has the *particular* intensity observed—also to account for the remarkable fact that the intensity diminishes in the complex ratio of the *square* of the distance, and not in some other ratio. Surely if any thing requires an explanation,

we have something to explain here. What, for example, would be thought of any one saying that the intensity of light varied as the *square* of the distance because it was its "*property*" to do so. The worst of this want of appreciation of the logical necessity for an explanation is, that the attention is called away and the inquiring faculties deadened, and thus these grand problems secure a share of attention which is utterly insignificant compared with that devoted to those of minor importance.

9. To prevent any misconception, we would remark here that the theory we have to suggest as an explanation of gravity is different in several essential points from that of Le Sage. The theory of Le Sage was dynamically defective in several essential points (probably owing to the comparatively small advance made in dynamics at his time). His assumption of continuous streams of particles coming from a number of different directions equiangularly scattered in space, the particles being supposed to come from *indefinite* distances ("ultramundane" particles), must appear evidently somewhat fantastic; for it appears inconceivable how the motion of such a system of streams of particles coming from ultramundane space should be kept up without confusion ensuing, owing to the mutual collisions of the particles of the streams which cross each other in all directions, if (as he assumed) each separate stream were *to move continuously in one direction*. For, however much the collisions might be reduced by reducing the size of the particles, they must occur in a long course of time, especially considering the high velocity at which it is necessary to assume the streams to move. Moreover the great objection to this view is that it involves, for the maintenance of gravity in the visible universe, a continual *supply* of matter from ultramundane space. This objection Le Sage distinctly recognized and could not surmount. The real merit of his theory was his fundamental idea that "gravity," or the tendency to approach of two masses, was due to the one mass sheltering or screening the other from the action of the streams of particles in which the two masses of matter were immersed— so that the remote sides of the two masses (where there is no shelter) are struck by a greater number of particles than the near sides (where there is shelter), and thus the two masses are urged together. The rest of his assumptions are in the nature of postulates, some of them unrealizable. He had little knowledge to draw upon at his time.

10. The points we have to bring forward are briefly as follows. We do not assume, as Le Sage did, the existence of streams of particles flowing as *continuous* currents in assigned

directions and coming from indefinite distances (or "ultra-mundane" particles, as he termed them). We do not assume that the particles producing gravity in the visible universe converge towards it in streams from ultramundane space. On the contrary, we assume that the matter producing gravity within the confines of the visible universe is *as a whole* at rest; or we regard the medium producing gravity simply as a gas. This gas differs from an ordinary gas only in the multiplicity of its particles, their excessive minuteness, and (consequently) extremely *long* free path. It is a direct consequence of the kinetic theory of gases that, *within the range of free path* of the particles of this gas, the particles move in precisely the right way to produce gravity; *i. e.* all the assumptions that Le Sage made *arbitrarily* as regards the motion of his streams, take place as inevitable necessities *within the range of free path* of the particles of a gas. The motion of the particles (in such a way as to produce gravity) is *automatically* kept up by a process of self-adjustment; i. e. *gravity is the inevitable result of the existence of a medium in space constituted according to the kinetic theory of gases.* It has been mathematically proved that the particles of a gas, within the range of free path, move uniformly or *equally towards all directions.* This special character of motion is automatically kept up under the influence of the collisions; or, however each particle (by itself) may change its course, this *general character* of motion is rigidly kept up, and is required to satisfy the condition of *equal* pressure in *all* directions. But this motion of particles uniformly or *equally in all directions* is precisely what is required for gravity.

11. The only further condition necessary is, that the range of free path of the particles should be great enough, so that (approximately) uninterrupted streams of particles move through the full range through which gravity has been observed to act. This length of free path (by any given number of particles in unit of volume) may be increased to any extent, simply by reducing the size of the particles. Taking, therefore, the visible universe as a whole, we have no streams of particles, but simply a gas at rest. The streams only exist within the range of free path of the particles, or within the range of gravity. We may compare the medium filling the visible universe to the air of a room, in which there are no streams, but the air is *as a whole* at rest. Contract the room (in imagination) up to the range of free path of the molecules of air, and we have streams of molecules sweeping in all directions through the room. The space in which we observe gravity may be compared to this contracted room, within

which streams of particles are sweeping through uniformly in all directions, the uniform motion of the particles *equally in all directions* (necessary to produce gravity) being *automatically* kept up under the influence of the mutual collisions, in a way demonstrated to take place in the case of a gas. It should be observed that this self-adjustment of their motion by the particles is not a mere result of chance, but a rigid adjustment of such a character that, if the uniformity of the motion were artificially disturbed, the particles when left to themselves would immediately correct the irregularity. The above length of free path, though great in one sense, becomes small and suitable for a gas pervading the vast range of the visible universe. Unlike Le Sage, we do not object to the collisions of the particles among themselves; for these collisions (in the case of a medium constituted as a gas) *maintain* the uniformity of motion. We require no *supply* of matter to produce gravity, and no supply of energy. The energy is self-contained. It is simply the case of the normal motion of the particles of a gas. Motion is as natural as rest. Nothing surely could be more simple than these conditions.

12. It might be said that this theory implies a *limited* range to gravity. It may be extended to any desired range simply by making the particles small, and consequently the free path great. We venture to think that rather than that a theory should be required to explain that the stars gravitate, a theory should be required to explain that they do *not* * gravitate. For surely the idea of an indefinitely extended universe all of whose parts gravitated towards each other, would represent dynamical conditions of *in*stability on the most gigantic scale. Imagine the incongruity of the idea of the whole universe tending to agglomerate in one (perhaps infinite) mass. To our mind no theory of gravity would be satisfactory that did not explain away this. The kinetic theory gets over this difficulty in a most complete manner, by allowing gravity to take place within a conformable range, without extending it to indefinite distances and thereby involving conditions of instability.

13. As we have said, we do not shirk in the slightest degree any criticism as regards this theory, but shall be glad to meet it, knowing that, if true, it will stand a full examination; and if false, the sooner it is proved so the better. There is one other point on which perhaps an objection might be raised. It might be said, If a gas exists in space, how is it that we do not detect its presence in experiments on the specific heat of other gases, this gas being at the *same time* present? or

* Of course we do not refer to *double* stars, in close range.

why does not some of the heat pass from the gas experimented on to this gas? In answer to this, it must be kept in view that the gravific medium, though in principle constituted as an ordinary gas, differs from an ordinary gas profoundly in several respects. First, it is necessary to assume that its particles are (as essential to the long free path) incomparably more minute than those of an ordinary gas, and the number of particles in unit of volume much greater. A molecule of an ordinary gas surrounded by the particles of the gravific medium, might be compared (as regards relative dimensions) to a visible mass surrounded by the molecules of air. Next, it is necessary to assume that the velocity of the minute particles of the gravific medium is incomparably greater than that of the relatively massive molecules of ordinary gases. Now, it is a known fact that the *resistance* to the passage of bodies through a medium constituted according to the kinetic theory *diminishes* as the normal velocity of the particles of the medium increases. By making, therefore, the normal velocity of the particles of the medium sufficiently great, all perceptible resistance to the passage of bodies through it will disappear. It is as if the medium did not exist; it becomes quite impalpable, or its presence impossible to detect. This is consistent with observation. The amount of energy, or motion, abstracted from a body passing through the medium, and given up to the medium, is exactly measured by the *resistance* encountered by the body. It is this transference of energy to the medium that constitutes the " resistance." If, therefore, there is no measurable *resistance* to the passage of the body through the medium, there is no measurable energy abstracted from the body. This gets over our difficulty ; for since the molecules of ordinary gases (at their relatively slow velocity) move through the gravific medium without appreciable *resistance*, there is no perceptible transference of energy (i. e. " *heat* ") from them to the gravific medium. In other words, the presence of the gravific medium cannot interfere with the experiments on the specific heat of ordinary gases. In short, the high normal velocity of the particles of the medium necessarily renders it in all respects completely *impalpable*, or its presence impossible to detect by the senses. The high velocity of the particles is only naturally adapted to the *minute* size of the particles.

14. It would seem difficult to avoid the application of the above principles to the case of molecules in close proximity— " cohesion " or " chemical union." For, first, it would appear obvious that molecules in contact would be urged together with *exceptional* force, owing to the parts in contact cutting

off the *entire* stream of particles*. Secondly, the *shapes* of diverse molecules (which would have no particular influence while the molecules were at a distance) would, when the molecules are in contact, have a great influence, according to whether the solid parts (or interstices) fitted over each other, so as to afford more or less shelter from the streams of particles. Possibly this might account for (or at least throw some light upon) the extraordinary *varied* behaviour of chemical "*affinity.*" If this were justified, it would certainly be a remarkably *simple* cause. It is just possible that a thing may be missed sometimes by looking too deep. The processes of nature are as a rule recognized to be *simple*, this being the necessary condition for order. "Simplicity is the soul of mechanics." This view, if well founded, would have the advantage of correlating *all* molecular actions (including "gravity") under *one* cause. We have thought it just as well to mention these views in passing (without attaching the same definiteness to them as we attach to gravitation).

15. We would in conclusion make a few remarks upon a matter of *principle* connected with this subject. It must be evident that under a dynamical theory of gravitation, when a mass is lifted, the energy expended in lifting cannot be converted into "*potential*" energy, but must be converted into *kinetic* energy, in imparting motion to the particles impinging upon the upper side of the mass, and which tend to urge it downwards. Conversely, when the mass falls, *kinetic* energy is transferred from the particles of the medium to the mass. As a general principle, therefore, by the abandonment of the theory of "*action at a distance,*" there can be no such entity as "*potential*" energy at all. We cannot avoid thinking that the very necessity to put forward a theory, that energy can possess, as it were, a *double* nature (kinetic, and *not* kinetic), in order to harmonize with the theory of "*action at a distance,*" is by itself a sufficient logical condemnation of this latter theory. The idea of "*potential*" energy (*i. e.* an energy which is *not* kinetic) involves the inconceivable idea of an energy *without motion*, *i. e.* a kind of *spiritual* energy, whose existence or non-existence leaves matter in the *same* physical state. Already serious doubts have been cast upon its validity as a logical principle by some of the most eminent minds. From the prevalent use of the term "*potential*" energy, and at the same time the common repudiation of the theory of "action at a distance," one would be inclined to draw the inference that there was an idea to a certain extent prevalent

* We believe Le Sage called attention to this in its application to "*cohesion.*"

that this term "*potential*" energy could still be used in a certain sense, even *after* the theory of action at a distance had been abandoned. We think it can be clearly shown that this is not legitimate. For, by the rejection of the theory of "action at a distance," external matter or a medium (in a state of motion) must be concerned in developing motion in matter; and therefore it must be a case of *kinetic* energy, not "*potential*" energy. Either (for example) the motion of approach of two masses (or molecules) is developed (as supposed) *without* the concurrence of external matter, or (secondly) this motion is simply transferred to the masses from external matter. In this latter case (which represents the case where the theory of "action at a distance" is rejected) the energy exchanged can only be the energy of motion (kinetic energy), *not*, therefore, "*potential*" energy. It might, perhaps, be urged that even when the theory of "action at a distance" is rejected, a raised mass can still be said to have "*potential*" energy (due to its position), because it *can* fall. This, however, may be proved not to be legitimate. For, from the very fact that (by the rejection of "action at a distance") the energy expended in raising the mass was converted into *kinetic* energy, it cannot have been converted into "*potential*" energy (*i. e.* an energy which is *not* kinetic) *as well.* A *double* equivalent of energy cannot be generated*. We think we have clearly shown, therefore, that by the rejection of the theory of "action at a distance," the idea of "*potential*" energy must (to be logically consistent) be unreservedly abandoned. The rejection of "*potential*" energy makes all energy of *one* character, viz. energy of *motion*; and then the great principle of the *indestructibility of motion* inevitably presents itself for acceptance. With the theory of "action at a distance," the idea of "*force*" (in the old sense of an action across space without the intervention of matter) must be given up. Thus we have in the physical world, only the two great fundamental conceptions of *matter* and *motion* left; or all physical phenomena come thus to be correlated in one grand and fundamental aspect, viz. as consisting in the various exchanges and phases of *motion*.

London, Jan. 11, 1878.

Note.—We think it right to add that we make no claim to have shown (as this had been already done by others) that the molecules

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* To say that a raised weight tending to approach the earth by the action of the gravific medium, possessed "*potential*" energy because it *can* approach the earth, would be like saying that a ship confined by a cable and tending to approach a rock by the action of the wind, possessed "*potential*" energy, because it *can* approach the rock (by the breaking of the cable). The cases are evidently parallel.

of a gas regulate their motions so as to move in a *particular* manner, though we doubt whether, if we had not arrived at this conclusion *independently* for ourselves, we should have been able to make a practical application of it. The point it has been our object to call attention to (and which apparently has not been noticed by others) is, that the motion of the particles of a gas *within the range of free path* precisely satisfies all the conditions Le Sage *arbitrarily* assumed in order to produce gravity—or that the special character of the motion Le Sage arbitrarily assumed his streams of particles to have, actually *exists* within the range of free path of the particles of a gas —in other words, that all the effects of gravity can be produced by *the mere existence of a gas in space*, and indeed *must* be produced if such a gas exists.

XVIII. *Electromagnetic and Calometric Absolute Measurements: the Absolute Value of Siemens's Unit of Resistance in Electromagnetic Measure; the Relation between the Current-work and the Heat-evolution in stationary Galvanic Currents; and the Absolute Values of some constant Hydroelectromotive Forces in Electromagnetic Measure. (Condensed Comparison of the Results of a Series of Investigations.) By* H. F. WEBER, *Professor of Mathematical and Technical Physics at the Federal Polytechnic Academy of Zurich.*

III. *The Heat produced by Stationary Galvanic Currents.*

MR. JOULE, thirty-seven years since, showed by experiment that the quantity of heat which a stationary galvanic current of intensity i generates in a conductor whose resistance is w, during the time z, is proportional to i^2wz. Sir W. Thomson then, in 1851 (and Prof. Clausius and others later), proved in the theoretical way that the value of the mechanical work which is expended in the stationary galvanic current of the intensity i, in a conductor with the resistance w, along which the electromotive force E is in action, in the time z is equal to the product iEz, or, pursuant to Ohm's law, equal to the expression i^2wz, where the quantities E, i, w are to be taken as measured according to absolute measure. If we make the assumption that, in a stationary galvanic current in which the evolution of heat is the only action of the current-flow, the amount of heat developed in the unit of time, Q, is the full equivalent of the work expended in the same time, then we have

$$JG = i^2w = iE,$$

where J denotes the mechanical equivalent of the unit of heat.

On this hypothesis, consequently, the proportionality-factor of Joule's law of heat-evolution is equal to the reciprocal value of J. Supposing that this assumption, the *whole* of the mechanical work consumed by the stationary galvanic current appears in the form of heat, is correct, we have a *new definition* for the absolute resistance of a conductor :—The absolute resistance (measured according to any system) of a conductor is equal to the mechanical value of the amount of heat which is generated in the conductor in the unit of time by the constant galvanic current 1 (measured according to the same system of measurement). And a *new method* for the experimental determination of the absolute resistance of a conductor:— Measure the amount of heat, Q, which in the time z is generated by the constant current i measured in absolute measure ; then the absolute value of the resistance (measured in the same system of measurement in which i is measured) is

$$w = \frac{JQ}{i^2 z}.$$

It cannot be maintained that the correctness of the hypothesis, "in the stationary galvanic current the *entire* work of the current is converted into heat," is so far above all doubt that one can without hesitation make use of the heat developed in a conductor by the stationary galvanic current for the absolute measurement of the resistance of the conductor. The results of the most exact investigations which have yet been instituted in this direction for testing the fundamental hypothesis in question contradict one another. Von Quintus Icilius (Pogg. *Ann.* vol. ci. 1856), in a carefully executed very extended series of operations, obtained the final result that the stationary galvanic current develops about 7 per cent. more heat than it should according to Thomson's equation ; on the other hand, Joule [*], in a comprehensive and very accurately executed investigation, with which he was charged by the British-Association Committee for the production of standards of resistance, found by experiment that in fact almost as much heat is produced in a conductor by the stationary galvanic current as is specified by the above-mentioned law. Von Quintus Icilius calculates from his experiments the mechanical equivalent of the unit of heat to be 399·7 metre-kilograms ; Joule infers from his the value 429·3 metre-kilograms for J (expressed in the *usual* measure of mechanical work). While the discrepancy between the results of these two series of observations is not cleared up the galvanic heat-

* Reports of Electrical Standards, edited by Jenkin, p. 165.

development cannot be unhesitatingly made use of for the absolute determination of resistance.

In order to procure the means to enable me to carry out an entirely unexceptionable determination of the absolute value of the S. M. U. by the heat-evolution of stationary galvanic currents, I undertook, in a very extensive and in many ways varied third experimental investigation, as rigorously exact a solution as possible of the question :—In a stationary galvanic current in which evolution of heat appears as the only action, is the heat generated in a certain time the *exact* equivalent of the mechanical work consumed by the current during that time ?

In the path of a current maintained constant, of which the absolute intensity i was carefully measured electromagnetically, was placed a thin platinum wire of about 15 S. M. U. resistance, wound in zigzag upon a numerously perforated frame of hardgum. Thick copper wires conducted the current to and from the platinum wire. The frame carrying the wire was suspended in a water calorimeter of the thinnest sheet copper, which was in an environment of constant temperature. The water filling the calorimeter amounted to about 250 grams ; the water-worth of the calorimeter-vessel, the frame, and the thermometer amounted to about 3 grams.

The constant current with the intensity i was conducted, during the time z, through the resistance w in the calorimeter. The mechanical work consumed by the current during this time, within the conductor with the resistance w, was then i^2wz. On the other hand, a certain amount of heat Q was generated in the resistance w, was given up to the calorimeter, and was to be calculated from the rises of temperature in the calorimeter, the water-worths of the substances filling the calorimeter, and the losses of heat of the calorimeter by radiation outwards or the gain of heat by the calorimeter from without. The mechanical value of this amount of heat, JQ, would necessarily, if the total work of the current were converted into heat, be equal to i^2wz.

On the hypotheses that the entire work of the current is converted into heat, that the exchange of heat between the calorimeter and its surrounding is governed by Newton's law, that the specific heat of water increases linearly with the temperature, and that the resistance of the platinum wire used increases proportionally with the temperature, the following differential equation holds for the dependence of the variable temperature t of the calorimeter on the time z:—

$$Mc_a[1+\gamma(t-t_a)]dt=\frac{i^2w_a}{J}[1+q(t-t_n)]dz-h(t-t_a)dz.$$

In this equation, M denotes the sum of the water-worths of the substances filling the calorimeter, t_a the constant temperature of the environment of the calorimeter ; c_a the specific heat of water, and w_a the absolute resistance of the platinum wire, at the temperature t_a ; γ the coefficient of the increase of the specific heat of water, and q the coefficient of the increase of the absolute resistance of the platinum wire, for 1° rise of temperature ; and h the heat which the calorimeter would part with to the outside if its temperature were 1° higher than that of its environment.

If we put $A = \dfrac{i^2 w_a}{JMc_a}$ and $B = \dfrac{Jh - (q-\gamma)i^2 w_a}{JMc_a}$ and assume that at the time $z = 0$ the temperature of the calorimeter is equal to t_0, the integration of the above differential equation gives the following connexion between the variable temperature t of the calorimeter and the time z :—

$$t - t_0 = \left(\frac{A}{B} - t_0 + t_a\right)(1 - e^{-Bz}) ; \quad . \quad . \quad . \quad . \quad (1)$$

or if the notion "mean temperature of the calorimeter during the time $z = 0$ to $z = z$" be introduced with the symbol $\bar{t}$,

$$JMc_a[t - t_0 + B(\bar{t} - t_a)z = i^2 w_a z . \quad . \quad . \quad . \quad . \quad (2)$$

The quantity $B(\bar{t} - t_a)z$ represents the temperature-correction which must be applied to the direct reading of the rise of temperature of the calorimeter on account of the heat-exchange with the environment, and on account of the variability of the resistance as well as that of the specific heat of water with rising temperature. This correction can be made as small as we please, by a suitable selection of the quantity $\bar{t} - t_a$. In all the measurements executed, care was taken so that this difference only amounted to so small a fraction of a degree that the correction, $B(t - \bar{t}_a)z$, to be added to $t - t_0$ amounted to only from $\frac{1}{300}$ to $\frac{1}{200}$ of $t - t_0$. The period z was chosen so great that the rise of temperature amounted to about 15°. For the determination of the mean temperature $\bar{t}$ of the calorimeter, and of the constant B, the temperature of the calorimeter was read off, from the commencement onwards, every five minutes ; in this way a series of equations of the form (1) were obtained, from which B could be ascertained. The thermometer of the calorimeter was most carefully compared, within its entire scale, with the air thermometer ; all readings taken from it were always reduced to the indications of the latter instrument.

The current-intensity i was measured in absolute electro-magnetic measure by means of the already mentioned simple tangent-compass ($R = 165 \cdot 7$ millims.) according to the relation

$$i = \frac{RH}{2\pi}\left(1 - \frac{3}{4}\frac{l^2}{R^2}\right)(1 + \theta)\tan u \; ;$$

for the measurement of u, mirror, telescope, and scale were made use of. To eliminate the daily variations of H, which on some days may reach $\frac{1}{5}$ per cent. of the mean value, H was determined, for the place of the tangent-compass, *before and after each* measurement. The variations of declination of the earth's magnetic force (which towards noon are very considerable for delicate measurements) were eliminated by regularly recurring, rapidly executed reversals of the current. A very powerful damper enveloped the magnetic needle of the compass, and permitted the readings of the deviations of the magnet to be taken again 20 seconds after the reversal of the current. The intensity of the current was maintained constant within $\frac{1}{500}$ or $\frac{1}{800}$ of its value by aid of a Dubois-Reymond's rheochord in the path of the current. The quantities l and θ were so small that the sum of the two corrections, $-\frac{3}{4}\frac{l^2}{R^2} + \theta$, amounted to only $+0 \cdot 0008$.

The absolute value of the resistance w was determined by the method described above in section II. As the temperature t_a of the calorimeter-environment varied somewhat from one day to another (up to $3°$), the coefficient of the increase of the resistance for $1°$ of rise of temperature had also to be known. To obtain the latter the absolute value of the resistance w was determined for the two temperatures (maintained constant) $0°$ and $23°$. At the same time the value of w for the same temperatures was measured in relative Siemens measure. The resistance of the platinum wire was found as follows:—

Temperature.	In absolute measure.		In relative measure.		Date.		
$22 \cdot 5$	$14 \cdot 498 \times 10^{10}\left(\dfrac{\text{millim.}}{\text{sec.}}\right)$		$15 \cdot 141$ S.U.		Oct. 14, 1876.		
$22 \cdot 9$	$14 \cdot 419 \times 10^{10}$	,,	$15 \cdot 142$	,,	,,	15,	,,
$23 \cdot 7$	$14 \cdot 486 \times 10^{10}$	,,	$15 \cdot 154$	,,	,,	16,	,,
0	$14 \cdot 141 \times 10^{10}$	,,	$14 \cdot 782$	,,	,,	17,	,,
0	$14 \cdot 121 \times 10^{10}$	,,	$14 \cdot 791$	,,	,,	18,	,,
0	$14 \cdot 130 \times 10^{10}$	,,	$14 \cdot 770$	,,	,,	19,	,,

K 2

For 23° the platinum wire possesses the absolute resistance

$$14 \cdot 468 \times 10^{10} \left(\frac{\text{millim.}}{\text{sec.}} \right),$$

and the relative resistance

$$15 \cdot 146 \text{ S. M. U. ;}$$

and for 0° the platinum wire possesses the absolute resistance

$$14 \cdot 131 \times 10^{10} \left(\frac{\text{millim.}}{\text{sec.}} \right),$$

and the relative resistance

$$14 \cdot 781 \text{ S. M. U.}$$

From the first result it follows that

$$1 \text{ S. M. U. } = 0 \cdot 9552 \times 10^{10} \left(\frac{\text{millim.}}{\text{sec.}} \right);$$

from the latter,

$$1 \text{ S. M. U. } = 0 \cdot 9560 \times 10^{10} \left(\frac{\text{millim.}}{\text{sec.}} \right),—$$

which are in perfect harmony with the results previously obtained in sections I. and II. We obtain the coefficient of the increase of the resistance, referred to 1° of temperature-increase :—

From the absolute measurements, $q = 0 \cdot 001035.$
And from the relative measurements, $q = 0 \cdot 001074.$

For the temperature t_a, employed in the experiment in question, the absolute value w_a was calculated according to the formula

$$w = 14 \cdot 131 [1 + 0 \cdot 001054 \, t] \times 10^{10} \left(\frac{\text{millim.}}{\text{sec.}} \right).$$

From the results adduced it follows that absolute determinations of resistance can be accomplished with such precision that the variability of the resistance with varying temperature can be ascertained from them very nearly as accurately as from resistance-comparisons according to the bridge-method.

In the course of the investigation a gradual alteration of the resistance of the platinum by the continual passage of currents through it was sought after with peculiar care. On the 16th October, 1876, at the temperature of 23°·7 the resistance was found equal to 15·154 S. M. U., or, reduced to 16°, equal to **15·032** S. M. U.

After the wire had served for twelve experiments, in which a current of absolute intensity 4 (a round number) passed through it during about an hour, it showed, on the 19th De-

cember, 1876, a resistance of 15·068 S. M. U., or, reduced to the temperature 16°, of **15·035** S. M. U. At the end of twelve more experiments, in which a current of about 6 absolute units passed through the wire each time during about 45 minutes, the latter showed, on the 28th March, at the temperature of 16°, the resistance **15·031** S. M. U.

Accordingly, under the influence of continual currents of absolute intensity from 4 to 6 the platinum wire underwent no demonstrable alterations. A special investigation showed that perceptible permanent alterations in the resistances of metallic conductors only make their appearance from a *definite* current-intensity onwards.

Without further remarks, the following are the results of the investigation.

Here also I varied the experiments in several ways. First, a series of twelve observations was instituted in which a proportionally feeble current passed through the wire in the calorimeter during a proportionally long period. From these twelve observations the following values were obtained for the mechanical equivalent of the unit of heat (the numbers are based on the ordinary measure of work; and with each is given the external temperature t_a to which the heat-unit on which the result is based refers):—

Date.			t_a. °	J. metre-kilograms.
October	20, 1876	....	16·6	428·49
,,	21, ,,	....	16·7	428·12
,,	26, ,,	....	16·3	425·51
,,	28, ,,	....	18·1	426·93
,,	30, ,,	....	18·5	429·93
,,	31, ,,	....	18·0	429·56
November 5,	,,	....	16·2	428·18
,,	6, ,,	....	16·0	427·28
,,	9, ,,	....	16·4	426·95
,,	15, ,,	....	17·1	428·50
,,	16, ,,	....	18·0	426·46
,,	20, ,,	....	19·1	427·19

Hence the mean mechanical equivalent J of the heat-unit is equal to **427·76** metre-kilograms (with a probable error of **±0·23**), if the specific heat of water at the mean temperature employed, $\bar{t}_a = 17°·2$, be put =1.

A second series of twelve measurements was next instituted, in which a proportionally stronger current was employed during a shorter time. The results obtained in this series were :—

Date.				t_a.	J. metre-kilograms.
December	21,	1876	....	19·8	428·36
,,	22,	,,	....	19·7	430·31
,,	23,	,,	....	18·7	426·37
,,	24,	,,	....	18·8	427·50
,,	25,	,,	....	18·8	427·45
,,	26,	,,	....	20·0	429·18
,,	27,	,,	....	20·1	428·02
,,	28,	,,	....	19·9	429·87
,,	29,	,,	....	19·4	430·15
,,	30,	,,	....	19·7	426·93
,,	31,	,,	....	19·5	427·90
January	1,	1877	....	19·6	428·96

According to this the mean mechanical equivalent of the heat-unit is equal to **428·42** metre-kilograms (with the probable error $\pm$ **0·25**), the specific heat of water at the mean temperature $\overline{t_a} = 19°·5$ employed being put $= 1$.

In a third series of experiments the period and the current-intensity were chosen such that the rise of temperature in the calorimeter amounted, as in the previous experiments, to about 15°. The proportions, however, were not so closely limited as to make the difference $\overline{t} - t_a$ as small as possible ; rather a play of a few degrees was given to it. The results of this series, in which the exchange of heat between the calorimeter and its environment possessed a value four or five times as great as in the previous series, were :—

Date.				t_a.	J. metre-kilograms.
March	28,	1877	....	16·1	427·15
,,	29,	,,	....	16·6	429·30
,,	30,	,,	....	16·8	429·61
,,	31,	,,	....	17·3	428·03
April	1,	,,	....	17·0	426·92
,,	2,	,,	....	17·7	428·56
,,	3,	,,	....	18·3	427·91
,,	4,	,,	....	18·0	429·10
,,	5,	,,	....	17·7	427·85
,,	6,	,,	....	18·9	427·52
,,	7,	,,	....	18·5	428·43
,,	8,	,,	....	17·9	428·93

According to this series the mean value of the mechanical equivalent of the unit of heat is **428·28** metre-kilograms (with a probable error of $\pm$ **0·18**), the specific heat of water, at the mean temperature $\overline{t_a} = 17°·6$ employed, being put $= 1$.

As the general result of these 36 tolerably accordant experiments (the extremes differing at the most only $\frac{1}{2}$ per cent. from the mean) we get :—The mechanical equivalent of the heat-unit, derived from the heat-evolution of the stationary galvanic current, has the value **428·15** metre-kilograms (with the probable error **±0·22**), understanding by unit of heat that amount which must be supplied to the unit of mass (1 kilogram) of water in order to raise its temperature 1° C. as measured by the *air thermometer*.

The surest means for deriving, in a purely thermic way, the quantitative value of the mechanical equivalent of the heat-unit is unquestionably furnished by the relation between the two specific heats of an ideal permanent gas—

$$J(c_p - c_v) = p_0 v_0 \alpha,$$

or

$$J c_p \frac{k-1}{k} = p_0 v_0 \alpha.$$

For atmospheric air the three quantities $p_0 v_0$, α, and c_p are very accurately known from Regnault's measurements : $p_0 v_0 = 7991$; $\alpha = 0·00367$; and $c_p = 0·23754$. The quantity k has been more recently determined for the same gas very carefully by M. Röntgen : $k = 1·4053$. Inserting these numerical values in the last equation, and also taking into account that, according to the experiments of Joule and Thomson, atmospheric air accomplishes in alterations of volume, besides the external work performed, an internal work equal to about $\frac{1}{500}$ of the external, we obtain from the thermal behaviour of air **428·95** metre-kilograms as the mechanical equivalent of the unit of heat. The unit on which this number is based is that quantity of heat which must be supplied to the mass-unit (1 kilogram) of water at 14° or 15° in order to bring about a rise of temperature of 1° (measured by the *air thermometer*).

Dr. Joule, in 1849, noted as the most trustworthy result of his numerous experiments on friction for the determination of the mechanical equivalent of the unit of heat the value $J = 423·79$ metre-kilograms. In the calculation of this number the specific heat of water was put $= 1$ for the temperature 14°·4 ; moreover the specific heat of the calorimeter-vessel was assumed too high. If the necessary correction on account of the latter circumstance be added, the result just mentioned becomes $J = 424·39$ metre-kilograms. The sixty friction-experiments made quite recently by Joule have given almost exactly the same result, 424·67 metre-kilograms.

Unfortunately the total result of Joule's friction-experiments,

J = 424·50 m.-k., cannot be compared at once with that obtained from the behaviour of gases, J = 428·95 m.-k. The two values are referred to quite different units: the basis of the former is 1° of Joule's mercury thermometer; that of the latter is 1° of the air thermometer. These two units may possibly differ 1 per cent. Perhaps, on reducing his previous and his recent friction-experiments to the indications of the air thermometer, Dr. Joule obtains a final result as good as identical with the value of J that follows from the behaviour of gases.

On account of this disturbing circumstance, I hold the value J = 428·95, drawn from the behaviour of gases, and immediately comparable with my above result, to be the most certain of those given by purely thermal determinations. Consequently, from the heat-evolution of stationary galvanic currents there comes as good as the same mechanical equivalent of the heat-unit as from purely thermal processes[*]. The hypothesis that the *entire* work consumed in the stationary current-flow appears in the form of heat has verified itself.

There still remains to say a few words on the already mentioned determinations, carried out by Joule and Von Quintus Icilius, of the mechanical equivalent of the heat-unit by galvanic heat-evolution.

Dr. Joule carried out 45 experiments, in three series[†]. He regards as the most trustworthy result that of the last series, comprising 30 experiments—J = 429·3 m.-k. In the calculation of this number the specific heat of water at 18°·4 was put = 1, and it was further assumed that the British unit of resistance in fact possesses the asserted value $10^{10} \left(\dfrac{\text{millim.}}{\text{sec.}} \right)$. According to our results this is not quite exactly the case: if the ratio of the British unit to Siemens's is as 1 : 0·9536, then the absolute value of the former is $= 1·0014 \times 10^{10} \left(\dfrac{\text{millim.}}{\text{sec.}} \right)$,

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[*] The two results, J = 428·15 (derived from the galvanic evolution of heat) and J = 428·95 (determined from the thermal behaviour of the permanent gases), refer, as was expressly remarked, to two different units of heat: in the former the unit is that quantity which can heat the unit of mass of water from 17°·5 to 18°·5; in the latter it is that which can heat the mass-unit of water from 14° to 15°. Therefore the two results will only then be strictly comparable, when the variation of the specific heat of water at variable temperature is *certainly* known. The experiments which I have, up to the present, instituted for fixing this hitherto totally uncertain quantity are not yet brought to a perfectly satisfactory conclusion. Yet so much can be positively known, that the reduction of the two values of J to the same temperature will bring about only a very slight alteration.

[†] Reports of Electrical Standards, edited by Jenkin, p. 175.

and Joule's result becomes 429·9 m.-k. Unfortunately, in this measurement also, Joule took for the basis the degree of the mercury thermometer, and not that of the air thermometer, and thereby made a precise comparison of his final result with ours impossible. Thus much may, however, be regarded as established, that so soon as Joule's mercury thermometer does not differ very considerably from the air thermometer, a tolerably good accordance exists between the results of the measurements made by Dr. Joule and by myself.

Von Quintus Icilius did not gauge the resistances made use of in his numerous measurements[*] according to absolute measure. The absolute resistance-values which formed the basis of his calculations he ascertained by a comparison of his resistances with the second copy of Jacobi's resistance-unit, produced by Wilhelm Weber for himself, and gauged by him according to its absolute value. This copy of Jacobi's standard was $= 0·9839$ of Jacobi's unit; and since, according to W. Weber's absolute-resistance measurements, the absolute value of Jacobi's resistance-unit is $= 0·598 \times 10^{10} \left(\frac{\text{millim.}}{\text{sec.}} \right)$, the copy had the absolute value $0·5884 \times 10^{10} \left(\frac{\text{millim.}}{\text{sec.}} \right)$. Von Quintus Icilius regards as the most trustworthy of his experiments the 34 in which water was employed as the calometric liquid. From these 34 experiments he calculates, as the final result, $J = 399·7$ m.-k. Singularly, this result has not in the least aroused the attention of physicists; and yet it was to be inferred therefrom, either that the measurements which conducted to it were very faulty, or that the theoretical views which formed its basis needed correction. The essentially different result obtained by me, in which by repeated trials I could detect no error, and the good accordance of which with Joule's results I could not but consider a further sign of its approximate correctness, caused me to reflect long upon the cause of the discrepancy; at last I succeeded in attaining a complete explanation:— *W. Weber, in his first absolute-resistance determination, found for the absolute value of Jacobi's resistance-unit about 8 per cent. too small a number*, in consequence of which Quintus Icilius's final result could not but come out just as much too little. If this error be corrected, the latter value (399·7 m.-k.) becomes **431·6** m.-k., a value which, certainly, is *somewhat greater* than that which results from Joule's measurements and my own experiments; but taking into consideration that Quintus Icilius has quite neglected the variation of the hori-

* Pogg. *Ann.* vol. ci. p. 65.

zontal component of the earth's magnetic force (which alone might make a difference of 2 units in the value of J), that he has not reduced the indications of the thermometer he employed to the air thermometer (a reduction which might make a difference of 4 units), and that he used in his experiments very powerful currents and very feeble resistances (a procedure which must necessarily have been attended with some slight errors), no great weight will be laid upon this small difference; the previously startling discrepancy is removed.

It can in two ways be shown that W. Weber, as we have maintained, found the absolute value of Jacobi's unit of resistance about 8 per cent. too small.

Bosscha, in 1856[*], determined according to Ohm's method the electromotive force of a Daniell's element in absolute electromagnetic measure. His measurements were based on a standard of resistance the absolute value of which, $0 \cdot 607 \times 10^{10} \left(\dfrac{\text{millim.}}{\text{sec.}} \right)$, was obtained by comparison with the above-mentioned copy by W. Weber of Jacobi's unit. He found the absolute electromotive force of a Daniell's element, in the mean out of several measurements,

$$= 10 \cdot 258 \times 10^{10} \left(\frac{\text{millim.}^{\frac{3}{2}} \text{milligr.}^{\frac{1}{2}}}{\text{sec.}^2} \right).$$

This result is proportional to the resistance taken as the basis of the measurement; the error committed in measuring this resistance enters into the derived value of the electromotive force.

From a long series of absolute measurements of the electromotive forces of the Daniell element, the details of which shall be related in another place, I have found that the *lowest* value of the electromotive force of the Daniell element in absolute electromagnetic measure is

$$10 \cdot 96 \times 10^{10} \left(\frac{\text{millim.}^{\frac{3}{2}} \text{milligr.}^{\frac{1}{2}}}{\text{sec.}^2} \right),$$

that the absolute value of the electromotive force of a Daniell's element of the form usually employed is

$$11 \cdot 30 \times 10^{10} \left(\frac{\text{millim.}^{\frac{3}{2}} \text{milligr.}^{\frac{1}{3}}}{\text{sec.}^2} \right),$$

and that the *highest* value of the electromotive force of a Daniell's element amounts to

$$11 \cdot 54 \times 10^{10} \left(\frac{\text{millim.}^{\frac{3}{2}} \text{milligr.}^{\frac{1}{2}}}{\text{sec.}^2} \right).$$

[*] Pogg. *Ann.* vol. ci. p. 517.

Which form of Daniell's element Bosscha used, unfortunately he does not state ; but we may assume as extremely probable that he made use of the form ordinarily employed, to which, according to my measurements, belongs the absolute electro-motive force

$$11{\cdot}30 \times 10^{10} \left(\frac{\text{millim.}^{\frac{3}{2}} \text{milligr.}^{\frac{1}{2}}}{\text{sec.}^2} \right).$$

This value is greater, in the ratio of 1·1016 to 1·0000, than that deduced by Bosscha. Now, supposing that Bosscha has carried out his measurements free from error (a supposition which of course cannot be rigorously correct), then the absolute value of the resistance taken by him as the basis of his measurements, *i. e.* the absolute value found by W. Weber for Jacobi's unit, would be 10·16 per cent. too little.

This calculation of the error is based on two somewhat un-certain assumptions, briefly indicated above. On this account it is a great advantage that an error in W. Weber's determi-nation of the absolute resistance of Jacobi's unit, in the same direction and of the same order of magnitude, can be deduced in quite another way. According to W. Siemens the ratio of Jacobi's resistance-unit to Siemens's is $= 0{\cdot}6618$. From our numerous and multifariously varied measurements the absolute value of the Siemens unit is $0{\cdot}9550 \times 10^{10} \left(\frac{\text{millim.}}{\text{sec.}} \right)$. Accord-ingly the absolute value of Jacobi's resistance-unit would be, from our measurements, $0{\cdot}6320 \times 10^{10} \left(\frac{\text{millim.}}{\text{sec.}} \right)$; while M. Wilh. Weber found only $0{\cdot}598 \times 10^{10} \left(\frac{\text{millim.}}{\text{sec.}} \right)$—that is, a value about 6 per cent. *less* than that found by us.

Hence the absolute measurement by M. W. Weber of Jacobi's resistance-unit has turned out *certainly* from 6 to 10 per cent. too little.

[To be continued.]

XIX. *Crystallographic Notes.*
By W. J. Lewis, *M.A., Fellow of Oriel College, Oxford*[*].

DR. HUGO MÜLLER had the goodness, some time ago, to send me some crystals of the isomerous compounds Quercite and Inosite, which he had obtained from new sources—the former from the leaves of the dwarf-palm (*Chamærops humilis*), and the latter from cochineal.

Quercite.—The crystallography of Quercite has been already determined by Sénarmont (Rammelsberg's *Die neuesten Forschungen in der Krystall-Chemie*); but it was a matter of interest to determine whether any difference either in habit or angles could be found in the crystals obtained from the new source. The crystals were found to show the same hemimorphous habit (fig. 1) observed by Sénarmont; and but a slight change has been made in the elements, which may probably be explained by the fact that the crystals obtained by Dr. Müller were very perfect.

The crystal is positive; the optic axes lie in the plane of symmetry; the mean line lies between c and g, and makes an angle of about 20° with the normal to g, the dispersion (*inclinée*) being considerable, $v > \rho$. The angles of the optic axes in air for the red and blue rays were found to be 55° 17′ and 58° 20$\frac{1}{2}$′ respectively.
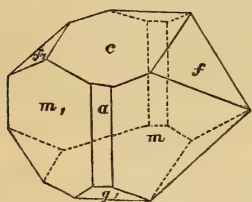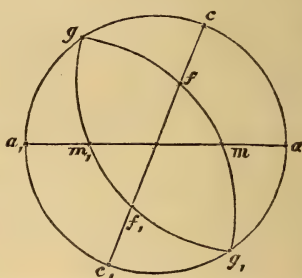
Fig. 1.   Fig. 2.

The forms observed are $a\{1\,0\,0\}$, $m\{1\,1\,0\}$, $c\{0\,0\,1\}$, $f\{0\,1\,1\}$, $g\{\bar{1}\,0\,1\}$ (fig. 2). The faces of the prism are striated parallel to their intersection with a; and there is a good cleavage parallel to $g\{\bar{1}\,0\,1\}$. The following are the elements and principal angles observed and calculated.

$$(1\,0\,0,\ 1\,0\,1) = 35° 32'\cdot2,\ (1\,0\,1,\ 0\,0\,1) = 33° 30'\cdot8,$$
$$(0\,1\,0,\ 1\,1\,1) = 66° 1'.$$
$$a : b : c = 1 : 1\cdot241 : 0\cdot95.$$

	Calculated.	Observed.	Sénarmont. Calculated.	Sénarmont. Observed.
$a\,c$	*69° 3′	69° 5′	68° 57′	68° 57′
$c\,g$	53 20$\frac{1}{3}$	53 16$\frac{1}{3}$		53 22 about
$g\,a_{,}$	*57 36$\frac{2}{3}$	57 35		57 20
$c\,f$	*35 33$\frac{1}{2}$	35 32	35 34	35 32
$f\,f_{,}$	108 53	108 49$\frac{1}{2}$	108 52	109 5

Table (*continued*).

| | Calculated. | Observed. | Sénarmont. | |
			Calculated.	Observed.
⎡ *a m*	36 57⅔	36 56¾		36 45
⎣ *m m,*	106 4⅔	106 6½	106 30	106 29
⎡ *g, m*	64 39½	64 43		
｜ *m f*	54 24⅙	54 21¼		
⎣ *f g*	60 56⅓	60 59		
⎡ *a f*	73 5⅓	73 8		
⎣ *f a,*	106 54⅔	106 59½		

Inosite.—This substance crystallizes in colourless, much striated prisms, attached by one end to the mass of the substance. The striations on the planes lying in the prism-zone rendered it impossible, even in the most delicate needles, to get reliable measurements of the angles in this zone. The prisms were terminated by four small planes, {1 0 $\bar{1}$}, {1 0 1}, and {0 1 2}, of which the former was most largely developed, sometimes even to the exclusion of the other planes. The crystals were extremely friable, and lost a portion of their water very readily—properties which rendered the examination difficult and prevented the determination of their optical character. The opposite faces in the zones were in all cases considerably displaced, so that there was always a divergence from the zone and from 180° in the sum of the angles between them. The following elements and measurements can therefore only be regarded as approximate.

Fig. 3. Fig. 4.
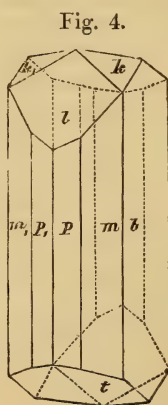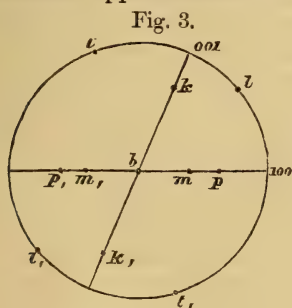
The system is oblique. The forms are *b*{0 1 0}, *m*{1 1 0}, *p*{2 1 0}, {4 1 0}, *l*{1 0 1}, *t*{$\bar{1}$ 0 1}, *k*{0 1 2} (fig. 4).

$$(1\,0\,0,\ 1\,0\,1)=40^\circ\ 51\tfrac{3}{4}',\quad (1\,0\,1,\ 0\,0\,1)=28^\circ\ 27\tfrac{3}{4}',$$

$$(0\,1\,0,\ 1\,1\,1)=62^\circ\ 45\tfrac{2}{3}';$$

$$a:b:c=1\cdot 0802:1:0\cdot 7869.$$

	Calculated.	Observed.
$l\,t$	71° 0′	70° 34½′ to 70° 27′
$l\,k$	34 24½	34 36 ,, 34 14
$t\,k$	46 15	46 11 ,, 45 54
⎡$b\,m$	44 42	44 50 ,, 44 36
⎢$b\,p$	63 11½	63 13 ,, 62 52
⎢$b\,(4\,1\,0)$	75 49½	76 0 ,, 75 45
⎣$m\,m_{\prime}$	89 24	89 37
⎡$m\,l$	57 51⅔	57 56
⎣$\overline{m}\,l$	122 8⅓	121 49½
⎡$m_{\prime}\,t$	74 49	74 58 ,, 74 52
⎣$\overline{m}_{\prime}\,t$	105 11	105 6 ,,105 10
⎡$m\,k$	61 24⅓	61 52 ,, 61 34
⎣$\overline{m}\,k$	118 35⅔	118 10
$m_{\prime}\,k$	89 17	
$b\,l$	90 0	89 53½
⎡$b\,k$	69 47½	69 32 ,, 70 3½
⎣$k\,k_{\prime}$ (over $0\,0\,1$)	40 25	40 13

Jordanite.—On a crystal of blende from the Binnenthal in the British Museum two small crystals, the one of Jordanite, the other of Binnite, are implanted. The former, on measurement, was found to be a combination of the forms $\{0\,0\,1\}$, $\{1\,1\,9\}$, $\{1\,1\,3\}$, $\{2\,2\,5\}$, $\{1\,1\,2\}$, $\{1\,1\,0\}$, $\{0\,1\,3\}$, $\{0\,2\,5\}$, $\{0\,1\,2\}$, $\{0\,2\,3\}$, $\{0\,1\,1\}$, $\{3\,1\,2\}$, $\{3\,1\,1\}$, $\{3\,1\,0\}$. Of these the forms $\{2\,2\,5\}$, $\{0\,2\,5\}$, $\{0\,2\,3\}$, $\{3\,1\,2\}$, $\{3\,1\,0\}$ are, I believe, new. The middle index in these symbols corresponds to the brachydiagonal usually denoted by the letter y and the parameter b. This arrangement is not to be confounded with that of Professor vom Rath, in which b corresponds to the makrodiagonal and a to the brachydiagonal. The angles between some of these planes observed and calculated from the elements, $c:\tfrac{1}{2}o=65^\circ\ 0'$; $\tfrac{1}{2}o:\tfrac{1}{2}o=50^\circ\ 49'$, given by Prof. vom Rath are:—

	v. Rath's notation.	Calculated.	Observed.
⌐001 : 013	$c : \frac{1}{3}d$	51° 33′	51° 33′
│ 001 : 025		56 30⅓	not determined.
│ 001 : 012	$c : \frac{1}{2}d$	62 6⅙	62 6
│ 001 : 023		68 20⅔	68 23
⌊001 : 011	$c : d$	75 10½	75 6
⌐001 : 312		74 24½	74 15
│ 001 : 311	$c : u$	82 3½	81 47
⌊001 : 310		90 0	89 55
⌐312 : 112		29 32	29 30
⌊112 : 012	$\frac{1}{2}o : \frac{1}{2}d$	25 24½	25 27

The plane (1 1 2) was the largest plane on the crystal, (0 0 1) the next. All the other planes were small; and some thin twin laminæ were observed intersecting the zones [0 0 1, 3 1 0] and [0 0 1, 1̄ 1 2].

Binnite.—This mineral has occupied the attention of several mineralogists, a summary of whose work on it is given by Hessenberg in his *Min. Notizen*, ix., where he describes a very beautiful specimen in his possession. Kenngott, after an examination of the crystals in Wiser's collection, came to the conclusion that the mineral was hemihedral, a conclusion combated by von Waltershausen. After a careful study of the distribution of the faces on his crystal, Hessenberg comes to a conclusion opposed to that of Kenngott; for although the forms {1 1 1}, {2 1 1}, {3 2 1}, {4 1 1}, and {1 0, 1, 1} were incomplete, he found that the faces of {1 1 1}, {2 1 1}, and {3 2 1} were present in an irregular manner. He has made no remark, however, on the fact that the faces of {4 1 1} and {1 0, 1, 1} are present in adjacent octants only.

In the examination of the specimen in the British Museum, especial attention was paid to the distribution of the faces of the different forms. It consists of two crystals united together in parallel positions, or possibly of one crystal whose free development has been prevented at one point by the presence of some body, and has the forms {1 1 0}, {2 1 1}, {1 0 0}, {1 1 1}, {3 2 1}, κ{4 1 1}, κ{6 1 1}, κ{7 1 1}, κ{1 0, 1 1}, and κ{2 3 3}, of which κ{7 1 1} is new. The forms {1 1 0}, {2 1 1} are well and about equally developed; the others are subordinate. The number of octants which could be examined was six; so that the question of the hemihedrism could be more thoroughly tested than it was by Hessenberg, who was only able to examine four. The forms {1 1 0}, {2 1 1} were well developed in adjacent octants, and are therefore holohedral. The forms

{4 1 1}, {6 1 1}, {7 1 1}, {1 0, 1 1}, and {2 3 3} were found in alternate octants only, and are consequently hemihedral. The faces of {3 2 1} were for the most part badly developed, and did not permit of any certain conclusion being drawn. Hessenberg found a plane of the form in each of two adjacent octants which excludes a hemihedrism with inclined faces. I believe it, from my observations, to be holohedral. A further examination of such crystals as are to be found in the various collections might possibly set the question of the hemihedrism of the mineral at rest, and would certainly be interesting.

XX. *Short Reports from the Chemical Laboratory of Trinity College, Dublin. By* J. EMERSON-REYNOLDS, *M.D., Professor of Chemistry, University of Dublin*.*

No. 5. *On the Rapid Estimation of Urea.*

A DISTINGUISHED physician, who wished to make frequent determinations of the urea daily excreted by a patient, requested me to devise a method which would enable him to make the desired estimation—(*a*) rapidly, (*b*) with sufficient accuracy for ordinary clinical purposes, (*c*) with simple and easily constructed apparatus, and (*d*) without the use of a balance or of any measuring-vessels other than the fluid-ounce and minim measures which a medical man is in the habit of employing.

This interesting practical problem was solved in the manner I shall presently describe; and the results obtained by the use of the method devised have been so satisfactory as to lead me to expect that it may be found generally useful where a high degree of accuracy is not desired.

I propose, however, before concluding this paper, to describe a less simple plan for the estimation of urea than that just referred to, but one which is capable of affording results of greater precision.

In both the methods mentioned I take advantage of the now well-known reaction of sodic hypobromite with urea. When a strongly alkaline solution of sodic hypobromite is added to a liquid containing urea, the latter suffers rapid decomposition into water, carbonic anhydride, and pure nitrogen gas. The carbonic anhydride is not evolved as gas, but is absorbed, with formation of sodic carbonate, by the free alkali of the liquid used to effect decomposition; the nitrogen is evolved in the gaseous condition, and its bulk determined either indirectly or

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* From the Scientific Proceedings of the Royal Dublin Society. Communicated by the Author.

directly, the volume of nitrogen produced thus serving as a measure of the urea from which it was derived.

The equation which expresses the change just referred to is the following :—

$$\underbrace{CO''N_2\,H_4}_{\text{Urea.}} + 3\,\underset{\text{Sodic}}{(NaBrO)} + 2\,(NaOH) = 3\,NaBr + Na_2\,CO_3$$
$$\underset{\text{hypobromite.}}{} \qquad\qquad\qquad + 3H_2O + 2N.$$
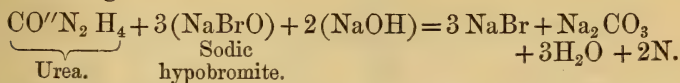
The use of calcic hypochlorite, or solution of "chloride of lime," in effecting a similar decomposition was pointed out by Dr. E. W. Davy[*]; and it has been recently shown by Yvon[†] that the hypochlorite used by Davy is more effective than the sodic hypochlorite, but it does not evolve the whole of the nitrogen and is irregular in its action. Knop, and after him Hüfner[‡], and many others, have shown that the sodic hypobromite is greatly to be preferred to any of the hypochlorites, as the decomposition of urea is almost complete, and progresses regularly and rapidly without the aid of heat : hence I use the hypobromite as the basis of the plan of operating now to be described, and in this respect agree with Hüfner, Russell and West, R. Apjohn, Blackly, Dupré [and with Simpson and O'Keefe §] in the methods they have proposed for ureaestimation.

The different methods devised by the above-named chemists all serve for the *direct* measurement of the volume of nitrogen evolved during the action of the hypobromite on urea, and involve the use of specially graduated tubes for the reception and measurement of the pure gas. My plan is essentially different, as the gas evolved, which is scarcely soluble in water ‖, is made to displace its own volume of that liquid, and the latter is then easily measured in any ordinary vessel, such as a tall and well-graduated drachm measure.

The apparatus may be most conveniently described as consisting of two distinct parts—A, the generating-vessel (see annexed woodcut, fig. 1), and F the small gas-holder, from which water is expelled by the nitrogen entering from A.

Gas-generating Vessel.—This is an ordinary two-ounce widemouthed bottle, fitted with a good india-rubber cork pierced with three holes. Through one of these holes the gas-delivery tube E passes, and through another the small piece of bent

[*] Journal of the Royal Dublin Society, and Phil. Mag. [IV.] vol. vii. p. 403.
[†] *Journal de Pharmacie et de Chimie*, [4] vol. xxiv.
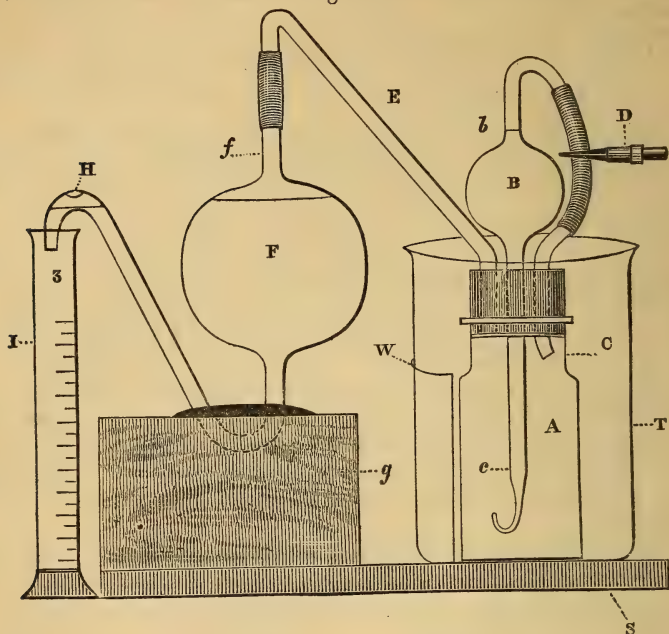[‡] *Journal für praktische Chemie*, [2] vol. iii. p. 1.
[§] Published since this paper was read.
[‖] According to Bunsen, water dissolves only 0·01478 of its volume at the mean temperature and pressure (Bunsen's 'Gasometry,' p. 286).

glass tube C. The heat afforded by a spirit-lamp suffices for
bending these tubes from straight pieces, and for the conversion

Fig. 1.

of an ordinary bulb-pipette, capable of holding rather more than
one fluid-ounce, into a vessel of the form B.

The delivery-tube of the pipette is first passed through the
remaining hole in the cork, and the end of the tube then drawn
out and recurved, as shown at *c*; the tube above *b* is bent so
far down as to admit of its being connected by means of an
india-rubber tube with the outer extremity of C. At D the
india-rubber tube is securely clipped by a small artery-forceps
with broad jaws. When the cork carrying the tubes just
described is secured in the bottle, the generating-vessel is
complete. When an estimation is in progress the bottle A is
placed in a tumbler, T (or beaker), containing cold water at a
temperature of 52° F., as nearly as possible.

The Gas-receiver.—This is easily constructed from a large
pipette whose bulb F is capable of containing about three fluid-
ounces. The tube *f* is cut off so as to admit of being securely
joined to the tube E of the generating-vessel by means of an
india-rubber connector. The delivery-tube is then bent, as
shown, and at the point H a little hole is made. A groove
cut in a block of wood *g* receives the bent tube of the little
gas-holder, which is then easily secured in its place by any

suitable cement—common sealing-wax, for example. Thus, with the aid of the two pipettes, cork, and tubing, which can be easily procured through a druggist for about 3s., a medical man can construct his own ureometer *.

When in use the block *g* is secured to the board S, on which the beaker T stands held by the wire W.

Mode of using the Apparatus.—The vessels A and F are disconnected, and F filled with water until it overflows and the excess has ceased to drip from the tube under H. The cork is removed from the bottle A, and two fluid-drachms of the liquid to be tested measured off in a tall minim-measure, and then poured into A ; one drachm of water is next used to rinse the liquid adhering to the sides of the measuring-glass into the bottle A : the total volume in A therefore should measure about three drachms †. For a reason which will presently appear, it is desirable that no more water than one drachm should be employed. If a pipette delivering two drachms be used, a drachm of water should be added ; but the pipette need not be rinsed with it. The next step is to fill the pipette B with the reagent which evolves the nitrogen of the urea. For this purpose a suitable vessel (a wine-glass for example) is filled with the hypobromite liquid ‡ ; the forceps D is removed from the india-rubber tube, but is placed close at hand, and a piece of vulcanized tubing, five or six inches long, attached to the end of the glass tube C ; suction is then applied by the mouth when the curved end *c* of the pipette is immersed in the hypobromite. The pipette is thus easily filled by suction with the re-agent up to the mark *b*. The forceps D is next applied to the connector, as shown, *before* the lips are withdrawn from the india-rubber tube attached to C ; the suction tube may then be removed from C, as the liquid is retained in B by atmospheric pressure, provided D pinches the tube well. Having washed the end *c* by pouring a little water over it, the cork carrying all its apparatus is securely inserted in the bottle A, the latter placed in the beaker T containing enough water to cover the cork when A is pressed down, and the tube E securely connected by the

* Messrs. Yeates & Son, of Dublin, supply a neat form of my apparatus ready for use.

† The measure used should be good, the two-drachm and two-ounce vessels agreeing with each other. The amount of reliance which can be placed upon the results depends in great part on the accuracy of the measures.

‡ This solution is prepared as follows :—Dissolve 4 ounces of the solid caustic soda of the shops in 10 fluid-ounces of water. When the soda has dissolved and the liquid cooled to 60° F., add gradually 1 fluid-ounce of bromine. The test solution is then ready for use. It should be kept in a cool place, and away from the light.

tightly-fitting india-rubber tube with *f*.   While connecting
the generator and receiver a little water is necessarily expelled
from the tube beyond H; but this water is thrown away, and
the *dry* two-ounce measure, I, then placed under the spout.

Up to this point the hypobromite has not been allowed to
come into contact with the urine ; but now, on removing the
forceps D, the hypobromite flows out from *c* and rapidly mixes
with the urine, the urea of which yields up its nitrogen gas with
effervescence.   As the gas evolved has no exit save through E,
it displaces from F its own volume of water, which falls into
the vessel I, and can then be measured when no more water
is expelled.   The effervescence ceases after five or ten minutes,
according to the temperature.

It is essential to good measurement that the pressure within
the apparatus should be the same at the end as at the beginning
of the experiment ; in order to secure this, the simple plan is
adopted of placing a wedge under the board S at the end in-
dicated, which is thus so tilted that the eye placed at a point
a little below D, and looking immediately above the surface of
the water in F, can just see the bend of the tube under H.
When the pressure within and without has been thus equalized,
the amount of water expelled is the measure of the nitrogen
evolved in A ; for we may in a test of this kind neglect the
extremely minute proportion of the nitrogen which has been
dissolved by the water.

When it is desired to correct for temperature and pressure
by means of the usual formula, it is now necessary to disconnect
E and *f*, and to pass the bulb of a small thermometer through
f into the gas over the water in F ; after a minute or so the
temperature may be read off and recorded, and the barometric
reading made at the same time.   In ordinary clinical ex-
periments, however, the correction for temperature may be
neglected when a thermometer in the room stands near to 52°
F.   The neighbourhood of a fire or stove must be avoided in
making the estimations of urea.

In measuring the water expelled we may either read off the
volume in drachms or sixths of a drachm ; but since ordinary
cylindrical two-ounce measures are rarely graduated to less
than half-drachms, the *best* plan is to pour the excess over a
definite number of drachms into a tall two-drachm measure,
bearing in mind that every ten-minim division represents the
sixth of a drachm.

I find as the results of a large number of direct experiments
with a standard solution of pure urea, some of which will be
given further on, that *one grain of urea produces sufficient gas
at a temperature of* 52° F. *and a barometric pressure of* 30·06

inches to expel 6⅘ *drachms of water*, the volume of liquid in the bottle A being three drachms, and the hypobromite added ten drachms.

It may be mentioned that measures of capacity need not be employed in the determinations of urea, as the water expelled may be received in any suitable vessel which has been previously weighed. At the end of the experiment the vessel and expelled water are weighed. When the estimation was made, under the conditions above named, *one grain of urea was found to expel, as a mean*, 365 *grains of water by weight*. This number is easily remembered, as it happens to be identical with the number of days in a year.

Effect of the Degree of Dilution upon the Determination of Urea.—An apparently trifling observation led me to examine the effect of dilution upon the yield of nitrogen obtainable from a constant weight of urea; and the results arrived at are stated in the Table given below.

The quantity of pure dry urea operated with in each of the following experiments was 2·222 grains; and the same volume (*i. e.* ten fluid-drachms) of a single sample of freshly prepared sodic hypobromite was added in each case. The experiments were completed within three and a half hours; and care was taken to avoid changes of temperature as much as possible : hence, while the barometer remained steady at 30·06 inches, the temperature varied within such very narrow limits (between 50° and 52° F.) that corrections for alterations of volume were unnecessary, as extreme accuracy in the measures of the water expelled was not attainable with the vessels advisedly employed, as I desired the results to be of such a kind as a medical man could easily obtain in his own study.

Table.

No. of experiment.	Pure dry urea used in experiment.	Volume of water used to dissolve urea in A.	Volume of water expelled from F.
1.	2·222 grains.	0 drachm.	15⅛ drachms.
2.	,,	1 ,,	15 ,,
3.	,,	2 drachms.	14⅝ ,,
4.	,,	2 ,,	14⅚ ,,
5.	,,	3 ,,	14¾ ,,
6.	,,	3 ,,	14⅔ ,,
7.	,,	4 ,,	14¾ ,,
8.	,,	5 ,,	Lost.
9.	,,	6 ,,	14 + ,,
10.	,,	7 ,,	13⅝ ,,
11.	,,	8 ,,	13⅚ ,,
12.	,,	9 ,,	13⅓ + ,,
13.	,,	10 ,,	13⅔ + ,,

The weight of urea taken for each experiment is capable of affording a volume of moist nitrogen gas at the temperature and pressure above stated, which would expel $15\frac{4}{5}$ drachms of water. The maximum volume obtained from that weight of pure dry urea was $15\frac{1}{6}$ drachms. Thus there is a minimum loss of 3·3 per cent. The maximum observed loss in the foregoing experiments amounted to 14·9 per cent., and occurred in the experiment in which the above-named weight of urea was dissolved in ten drachms of water. The loss within the above limits is tolerably regular, as the volume of nitrogen is diminished by $\frac{1}{6}$ of a drachm (nearly) for each drachm of water added to the urea in the decomposition-vessel A.

The loss of nitrogen referred to is, doubtless, due in part to solution of the gas; but it is chiefly attributable to the regular diminution of the strength of the oxidizing agent used, the hypobromite solution, and to a corresponding increase in the extent of secondary changes which are known to occur in the diluted liquids, and which involve a loss of gaseous nitrogen. Much of the error arising from the latter cause is avoided by adopting the plan of employing a constant volume of liquid; hence the recommendations already made that two drachms of urine should be measured into the bottle A, and the measure rinsed out with not more than one drachm of water. The total bulk of liquid in A ought then to measure as nearly as possible three drachms. Even when the sample to be tested is measured with a pipette, it is well to add one drachm of water from an ordinary measure in order to bring up the total volume of liquid to the amount recommended.

When the simple precautions are taken which I have already mentioned, the little apparatus described in this paper will enable a considerable number of estimations of urea to be made with rapidity, and with sufficient accuracy for ordinary clinical purposes. When very precise determinations are required, Liebig's process must be resorted to, as all the methods in which hypobromites or hypochlorites are employed are liable to the errors pointed out above; the accuracy of the results is also affected by the action of the reagent used on uric and hippuric acids, creatinine, and other nitrogenized compounds. On the other hand, when we desire to ascertain the total amount of nitrogen excreted by the kidneys, it is necessary to resort to the precise method of estimation which I communicated to the Surgical Society of Ireland[*].

* *Vide* Medical Press and Circular, May 13th, 1874, p. 402.

Estimation of Urea by direct Determination of the Nitrogen Gas evolved by Sodic Hypobromite.

The piece of appara-
tus now to be described
was exhibited at a meet-
ing of the Scientific
Club in 1871, and has
proved most useful for
the estimation of urea
by the hypobromite me-
thod, and, with a little
modification, for the
estimation of carbonic
acid in carbonates, and
for other similar pur-
poses. I shall only re-
fer at present to its use
in ureometry. In this
apparatus the nitrogen
evolved is directly mea-
sured as gas under con-
ditions which admit of
very accurate determi-
nations of volume in
cubic centimetres.

The apparatus is
shown in section in
fig. 2. The stand A
supports a tall glass
cylinder B. Through
the large india-rubber
cork which closes the
lower opening of the
cylinder the U-tube *c*
is passed, great care
being taken to avoid
breaking the small T-
connector *c*. The outer
limb of the U-tube is
provided with a glass
tap T. The limb within
the tall glass cylinder
is sufficiently wide to
contain 150 cub. cen-
tims. in the expanded
portion, which, in my

Fig. 2.
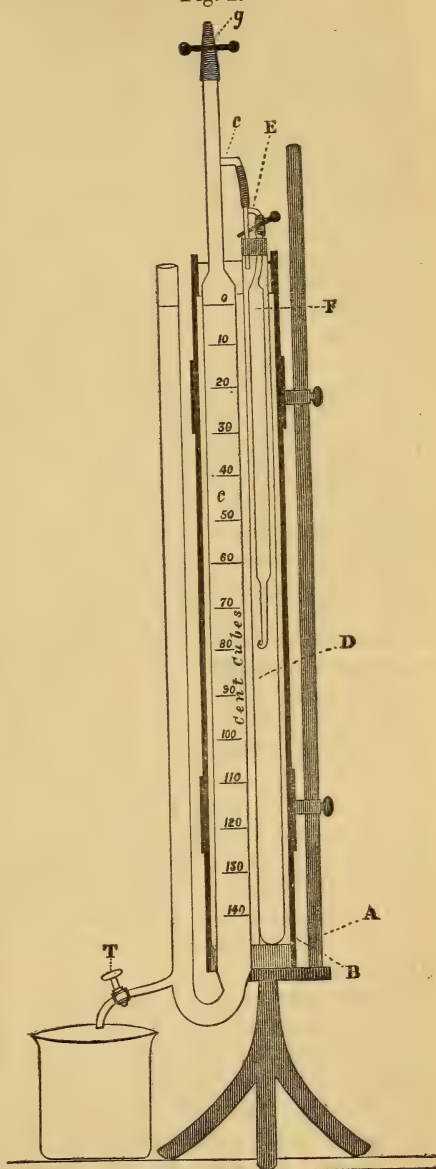
apparatus measures 60 centims. in length. The graduation cannot be conveniently carried beyond fifths of a cubic centimetre. At the point shown an india-rubber tube *g* is attached, which can be closed at will either by a good clip or by a stopper of glass rod. The glass side-tube *c* serves to connect the measuring-apparatus in the manner shown with the generating-vessel D, which is a long and wide glass tube placed within the cylinder. The glass T-tube E is connected by means of rubber tubing with *c*, while one limb passes through the india-rubber cork of D, and the other is connected by another piece of rubber tubing with the fine tube of the long pipette F (of about 20 cubic centims. capacity) which projects through the cork. This connexion must be sufficiently long to admit of the clip being applied as shown.

The large glass cylinder B is filled with water in order to maintain a steady temperature, the value of which can be known by means of a thermometer immersed in the water.

A determination is made with this apparatus in the following way:—Having disconnected the T-tube E from *c* and the clip, the generating-tube D is taken out of the water of the cylinder, the cork carrying the pipette, &c. withdrawn, and then 5 cubic centimetres of the urea solution introduced into the tube D. Before replacing the cork the pipette F is filled with hypobromite solution by suction above E, while the small glass tube opening on the underside of the cork is closed by a finger; the clip is then applied. The exterior of the pipette is now washed with a little water, and the cork, with the apparatus attached, is then replaced in position, the tube D again immersed in the water of the large cylinder, and the connexion between E and *c* securely made. Before making the connexion the water in the graduated tube should stand at the zero of the scale; but after making the joint the pressure within the apparatus is usually greater than that without. As the air in the tube D cools down to the temperature of the surrounding water, contraction takes place; but should the water not return to the zero, equilibrium is at once restored by opening the fine india-rubber tube *g* for a few seconds, and then closing in such a manner as to prevent any possible escape of gas.

The hypobromite is brought into contact with the urea solution by removing the clip from the india-rubber tube connected with the pipette; the reagent then falls from a considerable height and mixes thoroughly with the liquid at the bottom of the tube D. Nitrogen is evolved and displaces water from *c*, the water being maintained at the same level in both limbs of the U-tube by allowing the liquid displaced to run off by means of the tap T. When the evolution of gas has ceased,

the water-level is adjusted by means of the tap, and the volume of gas produced in the reaction then read off on the graduated tube c; the temperature of the water in the cylinder B is then ascertained, as well as the height of the barometer at the time. From the data thus obtained, the volume of dry nitrogen at $0°$ C. and 760 millims. can be easily calculated by the usual formula.

XXI. *Notices respecting New Books.*

Experimental Researches in Pure, Applied, and Physical Chemistry. By E. FRANKLAND, *Ph.D., D.C.L., F.R.S.* London : John Van Voorst. 1877.

DR. FRANKLAND has laid his chemical brethren under a great obligation by the publication of his researches in a collected and classified form. That obligation may be best repaid by the determined efforts of other chemists to explore those fields of knowledge which have been left untrodden by the author of the work now before us.

The researches of the South-Kensington Professor extend over a period of about thirty years ; they are arranged in three sections —Pure, Applied, and Physical Chemistry.

In the division of Pure Chemistry, Dr. Frankland has rendered himself famous by his researches upon the Alcoholic radicals, Organo-metallic bodies, and Synthesis of the Acids of the Lactic, Acrylic, and Acetic series. At the time when the earlier of these investigations appeared, the chemistry of the Carbon compounds was in a state of confusion : many facts had been collected, but little breath of life had been breathed into these dry bones. Laurent and Gerhardt had scarcely made known the results of the application of their brilliant classificatory powers to the facts of organic Chemistry. The theory of radicals had indeed been advanced by Liebig and Kane ; but the unscientific use of hypotheses concerning the nature of organic compounds was yet, for the most part, dominant. Berzelius and his dualistic theory were masters of the field. In terms of this theory, Berzelius viewed Acetic Acid as a conjugated compound containing the groups $C_2 H_3$ and $C_2 O_3$ (old notation). It is worthy of remark that the exceedingly imperfect and one-sided theory of Berzelius, as applied in the above-cited case, should have furnished an idea which, when worked out by Dr. Frankland in his researches upon the " Conversion of Cyanogen into Oxatyl," led to results of much importance in advancing the more complete and more probable theories of modern Chemistry. The Radical theory of Liebig found great support by the publication of Frankland's memoirs upon the "Isolation of the Alcoholic Radicals." In attempting to isolate the radicals Methyl, Ethyl, and Amyl by the action of metals upon the iodides of these bodies, Frankland obtained results which he then regarded, and which, judging from the introductory remarks in the present volume, he seems still inclined to regard, as proof of the actual isolation of these radicals. Frankland pointed out the analogy between Hydrogen and the radicals of the Alcohols ; and, if the molecular formula of Hydrogen be H_2, he

argued that the molecular formulæ of Methyl and Ethyl should be $(CH_3)_2$ and $(C_2H_5)_2$ respectively. But the hydrides of these radicals have respectively the formulæ CH_3H and C_2H_5H: Schorlemmer has shown that C_2H_5H is identical, not isomeric, with Frankland's Methyl $(CH_3)_2$. Frankland's own investigation upon the action of Chlorine upon Methyl (?) and upon Ethylic hydride, led him to regard these bodies as isomeric only. We had supposed that Schorlemmer's investigations had finally settled this point; but from what Frankland says in the present volume, he, at least, does not appear to regard the evidence as perfectly conclusive either way. Had the researches upon the Isolation of the Alcoholic radicals been productive of no other effect than to incite other chemists to attack the problems which they enunciated, they would have deserved the warmest thanks of every student of chemical science. But they did more than this : in these researches a great number of new and most important facts were added to the science ; new instruments and new methods of research were introduced to the chemist ; and new generalizations were advanced, which have most powerfully aided in the advancement of the true scientific study of the carbon compounds, notwithstanding that some of them have been unable to withstand the criticism, and have failed to completely explain the facts amassed by later investigators.

The analogy between Frankland's Alcoholic radicals, $(CH_3)_2$, $(C_2H_5)_2$, &c., and Hydrogen (H_2) doubtless led to important generalizations ; it may, however, it seems to us, be pushed too far. If the molecule be the smallest part of a body which exhibits the properties of that body, then $(CH_3)_2$ and $(C_2H_5)_2$ may be the molecular formulæ of methyl and ethyl respectively. In entering into chemical action, these molecules may be regarded as splitting up each into two atoms, CH_3 and C_2H_5, just as we regard the molecule H_2 as being divided into H H before a chemical combination takes place between hydrogen and another body. But if this be a true hypothesis concerning the molecular and atomic constitution of Methyl and Ethyl, it would almost necessitate the view that, after all, transmutation or something analogous thereto is actually a fact, because the atom (if one may speak of the atom of a compound) CH_3 combines with another atom CH_3 to produce the *same* compound as is formed when the atoms C_2H_5 and H combine together.

At the beginning of the volume now before us is placed a paper, first published some ten years ago, on Notation. We cannot but regard it as a mistake on the part of Dr. Frankland that in collecting his researches he has maintained that peculiar notation which, with its thick and thin letters, with its small O's and large C's, has never met with favour among chemists in general, and the presence of which in the present work must surely somewhat narrow the influence for good of these memoirs. This notation is founded on many somewhat sweeping generalizations : it really, we think, assumes an amount of knowledge which we do not possess ; and in doing this it tends, we are afraid, rather to hinder than to advance the progress of true inquiry.

Dr. Frankland is an upholder of the doctrine of varying valency. In his paper on Notation he brings forward the unhappy example of Ammonium Chloride as illustrative of compounds in which Nitrogen is pentavalent: in Nitrous Oxide he says Nitrogen is monovalent. Surely such statements as these are far too rash: we do not know the molecular formula of sal ammoniac; and we are much more justified in saying that nitrogen is trivalent in nitrous oxide, than in averring, as our author does, that the same element is trivalent in nitric oxide. Variation in valency seems to take place by leaps: a trivalent element may, it seems, sometimes act as monovalent, but rarely, if ever, as a divalent element. Such facts, says Frankland, " can be explained by a very simple and obvious assumption, viz. that one or more pair of bonds belonging to one and the same atom of an element can unite, and, having saturated each other, become, as it were. latent." We fail altogether to see that this " simple assumption " *explains* the facts in any way; it merely restates them. On pp. 24, 25 there appears a most unfortunate list of mineral compounds formulated in accordance with Dr. Frankland's system. The formulæ there given are pleasant to the eye; but that is all. For the most part they really have no *known* foundation in fact.

Section II. includes those papers which have been contributed by Dr. Frankland to Applied Chemistry: these papers chiefly deal with subjects connected with Gas and Water. The practical results of the work collected in this section are known to all. We are certainly largely indebted to the author of these researches for many improvements in our lighting and in our water-supply. The memoirs now collected form a good example of the benefits which always accrue when science is adequately applied to technical subjects. They may furnish a powerful argument to those who are ever urging Government to expend a larger portion of the nation's money in investigations undertaken by really qualified scientific men into those problems of applied science which every one acknowledges must be solved, but which can only be solved by national effort.

The papers upon Water-analysis recall the controversy between the upholders of the system of Wanklyn and those of the system of Frankland. Such a controversy should never have occurred: happily there are signs that the bitterness is dying away; let us hope that the action of time will be as the action of a flowing river upon the subject which has engendered this hostility, and that before long the clamour may be remembered only as a dream of " previous sewage contamination."

In the Third Section we are presented with the papers on Physical Chemistry contributed by Dr. Frankland to science: these papers, for the most part, contain the record of work done or suggested during holiday excursions. " The Influence of Pressure upon Combustion," the " Spectra of Gases and Vapours," the " Source of Muscular Power," and " Climate " form the main subjects dealt with in this section. The results of Dr. Frankland's researches in these various fields have passed into the common stock

of knowledge. Some of these results have been called in question, more particularly those concerning Flame. The recent researches of Heumann have, we think, shown that Frankland was much too sweeping in many of his assertions regarding the luminosity of hydrocarbon flames, and have proved that those flames do indeed owe their luminosity to the presence of solid matter. Nevertheless Frankland's experimental work on Flame remains a monument of what may be done even in the time given to recreation by a determined and loving student of Nature.

The papers on Muscular Power embody the results of much accurate and exceedingly valuable work; they are of interest both to the chemist and to the physiologist.

The publication of these collected researches cannot but increase the fame which their author has already earned; it cannot but aid in the advance of scientific chemistry by setting before the student an example of what may be accomplished by steady honest work, and by instructing him, both by precept and example, in the path of true scientific research. The best return which can be made to Dr. Frankland is that every worker in the field of chemistry should determine that he too will prove himself not unworthy of that study to which he has devoted himself.

XXII. *Proceedings of Learned Societies.*

GEOLOGICAL SOCIETY.

[Continued from p. 74.]

December 5, 1877.—Prof. P. Martin Duncan, M.B., F.R.S.,
President, in the Chair.

THE following communications were read:—

1. " On the Building-up of the White Sinter Terraces of Roto-Màhànà, New Zealand." By the Rev. Richard Abbay, M.A., F.G.S.

The author described the structure and mode of formation of the so-called " White Terrace " of Roto-Màhànà, which is produced by a deposit of silica from the water of a geyser situated on the side of a small hill of rotten rhyolitic rock, about 100 feet above the surface of the warm lake (Roto-màhànà), into which the water from the geyser finally flows, and the foot of the siliceous terrace projects. The geyser-basin, which is between 300 and 400 feet in circumference, has steep walls, broken through only on the side towards the lake, where the water pours down to form a succession of terraces, which are really shallow basins, over the outwardly inclined edges of which the water flows, depositing the dissolved silica in a white subflocculent form on the edges and bottoms of the basins in proportion as the water cools. The author showed how this arrangement produced the peculiarly formed siliceous deposit of the terraces, and that, as the growth of the latter is evidently upwards as well as outwards, it seems probable that the geyser-pipe has slowly worked its way up the hill by the solvent action of the heated water, from the level of the lake to its present elevation.

2. "Additional notes on the Dimetian and Pebidian Rocks of Pembrokeshire." By Henry Hicks, Esq., F.G.S.

The additional facts communicated by the author show that at a distance of about 10 miles to the east of the Dimetian axis of St. David's there is another ridge of these rocks, which also runs nearly parallel with it. This is also flanked by Pebidian and Cambrian rocks, and made up of rocks like those in the St.-David's axis.

The *Dimetian* formation, so far as it is at present known, consists chiefly of the following rocks :—

1. Quartz porphyries, containing frequently perfect quartz crystals (double pyramids), subangular masses of quartz, and crystals of felspar in a felspathic matrix.

2. Fine-grained greyish quartz-rocks, very compact, and inter-stratified with the above.

3. Ashy-looking shales of a dull green colour, sometimes highly indurated, but usually showing lines of lamination. Microscopically these show basaltic characters, and are probably greatly altered interbedded basaltic lavas.

4. Compact granitic-looking rocks.

5. Quartziferous breccias.

6. A series of compact quartzites and crystalline schists, inter-stratified with green and purple altered basaltic lavas with a slaty and schistose foliation, and with some dolomitic bands.

Of the *Pebidian* formation new areas were added, and the portions described in the author's previous paper were further extended, and details as to the chief mineralogical characters given. At the base of the series resting unconformably on the Dimetian is seen an agglomerate composed of large angular masses of a spherulitic felstone, pieces of quartz and quartzites, indurated shales, crystalline schists, &c., cemented together by a sea-green matrix of felstone. These are followed by conglomerates of the same materials, which are again succeeded by indurated shales, often highly porcellanitic in character, with a conchoidal fracture.

These are followed by a thick series of silvery-white and purplish shales and green slates, alternating with fine and rough ashes, often conglomeratic, hornstone breccias, felstone lavas, &c.

The series, as exhibited at St. David's, has a thickness of over 8000 feet ; and as it is everywhere, so far as yet seen, overlapped unconformably by the Cambrians, it may probably be of much greater thickness. It evidently consists very largely of volcanic materials, at first derived from subaerial, but afterwards from submarine volcanoes. These materials, however, were also undoubtedly considerably aided by sediments of a detrital origin.

The whole series shows that the sediments have undergone considerable changes, but yet not sufficient to obliterate the original characters, and the lines of lamination and bedding are usually very distinct. That they were altered nearly into their present state before the Cambrian sediments were deposited upon them is clear from the fact that the pebbles of the Cambrian conglomerates which rest immediately on any portion of the series are almost invariably made up of masses of the rocks below, cemented by gritty materials on an unaltered matrix, and from which the

pebbles may be easily removed. The great conglomerates at the base of the Cambrians, everywhere in Wales, indicate that there were beach- and shallow-water conditions over those areas at the time, and that the sea was then encroaching on an uneven land, becoming gradually depressed to receive the subsequent Cambrian sediment.

XXIII. *Intelligence and Miscellaneous Articles.*

THREE EXPERIMENTS WITH TELEPHONES.
BY PROFESSOR E. SACHER.

IN order to ascertain what were the feeblest induced currents in the telephone that would be sufficient to produce in the ear distinct sensations of sound, on December 27, 1877, I made the following experiments.

1. I led the closed current-circuit of the telephone, at 20 metres distance, parallel with the wire, well insulated with linen and wax, of an ordinary telegraph-apparatus. The signals were given at first by six, and afterwards by three Smee's elements ($\frac{1}{20} H_2 SO_4$); and they were, through the induced currents in the telephone, so distinctly audible that the message (by the two signals, long and short) could be understood.

2. I effected a division of the current by uncovering two places in the insulated wire distant from each other 20 metres, and fixing there the ends of a telephone-wire of equal strength and 120 metres long. Reckoning, in addition, the thin wires in the interior of the telephone, certainly only a small portion of the current went through it; and yet the rap was perceived with sufficient distinctness for the message this time also to be understood. (Hence it is easy to tap the messages of any open telegraph-line if one can learn to read the Morse alphabet by hearing.)

Experiment 1 succeeds also when the telegraph-wire is connected with the thick, and the telephone-wire with the thin wire of a Ruhmkorff. If we wish to perceive the signals more distinctly, it is advantageous to appropriate two telephones to the hearing; it is then, moreover, better to close the other ear against external noises.

3. I connected the telephone-wire, about 40 metres long, with the inner, thick wire of an ordinary induction-coil, and the wire, 120 metres in length, of a second telephone-system, with the outer thin wire. To my great surprise, we could correspond both from the first to the second telephone, and also (indeed apparently still better) in the reverse direction, nearly as well as with the connexion direct. The words were heard still more distinctly when I inserted in the same manner two induction-coils. On the contrary, the experiment was a failure when a Ruhmkorff was employed in this way; the sound was too faint.—*Kaiserliche Akademie der Wissenschaften in Wien, math.-naturw. Classe,* January 3, 1878.

ON THE LIQUEFACTION OF HYDROGEN. BY RAOUL PICTET.
(IN A LETTER TO M. DUMAS.)

Geneva, Jan. 11, 1878.

I have the satisfaction of communicating to you the result of an experiment made on Thursday, January 10th, consisting in the

liquefaction and solidification of hydrogen. I made use of exactly the same apparatus as for the liquefaction of oxygen, employing protoxide of nitrogen instead of carbonic acid.

To obtain the hydrogen under pressure, I employed the decomposition of formiate of potass by caustic potass. The hydrogen was liberated without any trace of water, and the residue is not volatile—two conditions essential for the rigorous accuracy of the observations. The temperature of the reaction is well defined, and did not rise. The liberation of hydrogen proceeded with perfect regularity. The pressure reached 650 atmospheres before becoming stationary. The hydrogen disengaged corresponded to 252 litres at zero. The cold was about $-140°$ (I have not yet effected the reduction of the measurement of the temperature). When I opened the stopcock, liquid hydrogen issued with vehemence from the orifice, producing a sharp hissing sound. The jet had a steel-blue colour, and was perfectly opaque for a length of about 12 centimetres. At the same time a rattling was heard upon the floor like the noise made by hail falling upon the ground, and the hissing was changed into a whistling which resembled that heard when a piece of sodium is thrown upon water. Almost immediately, the jet became intermittent, and shocks were felt in the cock at each issue.

During the first stream the pressure fell from 650 atmospheres to 370. After closing the cock the pressure diminished gradually during several minutes down to 215 atmospheres; it then rose again slowly up to 225, at which it again became stationary. I re-opened the cock; but the jet issued in such an intermittent manner that it was evident hydrogen had congealed in the tube. This hypothesis was demonstrated by the progressive exit of all the hydrogen when I had stopped the pumps and the production of cold. I explain the difference between these results and those which I obtained for oxygen as follows :—

The atomic weight of hydrogen is $\frac{1}{16}$ of that of oxygen; therefore the latent heat of liquid hydrogen must be certainly ten times that of oxygen. As soon as the exit-cock is opened a portion of the liquid stored in the tube evaporates, absorbing such an amount of heat by this change of state that the rest solidifies in the tube, even before it can be driven out.

During more than a quarter of an hour we had successive discharges of hydrogen through the orifice. The fog produced by the sudden expansion of the gas at the commencement of the experiment descended as far as the ground; but it ceased completely as soon as the jet became intermittent, which corresponded to the congelation of hydrogen in the interior of the tube. It is impossible to confound the vesicular fog of the gas with the appearance of the liquid jet at the outset. These different appearances are perfectly distinct and give rise to no ambiguity.

I know the volume of the residue, which is only carbonate of potass; and I shall be able in the next experiment to determine the density of liquid hydrogen.—*Comptes Rendus de l'Académie des Sciences,* Jan. 14, 1878, tome lxxxvi. pp. 106, 107.

ON THE ELECTRICAL AFTERCURRENTS OF TRANSVERSELY
MAGNETIZED IRON RODS. BY PROF. H. STREINTZ.

The experiments referred to in this paper were carried out by the author in conjunction with Dr. F. Streintz.

The phenomenon was discovered by Villari, that in a rod of iron or steel through which a galvanic current has passed, when agitated after the interruption of this current, a galvanic current is again generated the same in direction as that originally conducted through the rod. He subsequently investigated some properties of these aftercurrents, and gave an explanation of the phenomenon:—namely, that the rod is transversely magnetized by the current; and if we employ the notion of molecular magnets, these arrange themselves in concentric circles around the axis of the rod. Now, if the rod be agitated after the interruption of the current, the molecular magnets obey the direction-force which tends to mingle them again irregularly. We can here avail ourselves of the representation of the molecular magnets returning to the positions which they had before the action of the current. Now, in this return they generate in the rod an induction-current, to which Villari gives the name of "agitation-current," but which may with equal suitableness be designated as an aftercurrent.

Afterwards H. Herwig studied the properties of transverse magnetism on iron tubes.

The author now shows that in a very simple manner the quantity of the magnetizing force can be calculated which is exerted upon the molecular magnets by the current originally conducted through the rod. Starting from Biot and Savart's theorem, that a rectilineal current of infinite length acts upon a pole of a magnet with a force inversely proportional to the perpendicular distance of the pole from the conductor, the problem is reduced to one of the plane; so that we have to calculate the action of a circular surface with uniform mass (the rods investigated had a circular cross section) upon a mass-point situated in the surface.

In accordance with the laws of force previously stated, however, a circular line with uniform mass exerts no action on a point situated in the enclosed surface, while it acts on an external point in the same plane as if the mass of the circle were collected in the centre.

Now by this the calculation becomes very simple, and we obtain, as the force which is exerted upon a magnetic pole situated at the distance r from the axis, $p = \dfrac{kr}{a^2}$, in which k is a constant, and a the semidiameter of the rod. The total moment upon all the molecular magnets contained in the rod is then $R = Kla$, where K, again, is a constant, and l denotes the length of the rod.

The author has also investigated by experiment the properties of the aftercurrents; and in so doing he found some confirmed which could be foreseen from the theoretical developments, and also discovered various other properties, some of them indeed very striking, which could not have been determined *à priori.*—*Kaiserliche Akademie der Wissenschaften in Wien, math.-naturw. Classe,* Dec. 13, 1877.

XXIV. *On Reflection of Polarized Light from the Equatorial Surface of a Magnet. By* JOHN KERR, *LL.D., Mathematical Lecturer of the Free-Church Training College, Glasgow.**

IN trying to carry forward the magneto-optic inquiry which formed the subject of my last communication to this Magazine †, I proceeded to examine a lateral face of an intensely magnetized iron bar as a reflector, and had the pleasure of obtaining good effects in the first trial. I have lately performed a series of careful experiments on the subject; and I propose to give an account of these and of their very interesting results in the present paper. I mean to describe the experiments at sufficient length, for the guidance of any one who would like to repeat them. Most of them are, I think, rather easier and more satisfactory than those described in my former paper.

1. *Apparatus.*—The electromagnet is the same upright horseshoe that was used in my former experiments, a small but effective instrument constructed by Mr. Ladd. Each coil contains about 200 turns of thick double wire. The coils are put into circuit, through a good Ruhmkorff's commutator, with a series of six Grove's elements, the connexions being made in the usual way as for magnetization of the horseshoe.

The reflecting bar is a rectangular prism of soft iron, 7 inches long, 2 wide, ¾ thick. The iron was selected and specially forged; and its structure is homogeneous and very

* Communicated by the Author.

† "On Rotation of the Plane of Polarization by Reflection from the Pole of a Magnet," May, 1877.

fine. One lateral face of the bar (7 by $\frac{3}{8}$) was planed and carefully polished by a skilled workman.

It may be worth mentioning that three such bars were forged at the same time, were cemented together, planed and polished in block, and then separated. The middle one was kept as the best reflector ; but most of my results were verified upon each of the three.

2. *Arrangements.*—The electromagnet stands upright upon a solid table ; the reflecting bar lies flat and stably on the poles of the horseshoe, in the position of an armature, its length horizontal, and its polished face vertical ; the two Nicols and the lamp stand upon the same table as the magnet, and at the same height as the mirror. The diagram shows all the pieces, in horizontal section through the lamp L and the
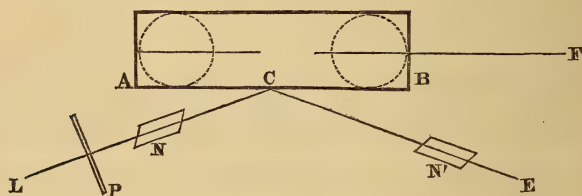
observer's eye E.   N is the first Nicol, C the point of incidence on the reflector AB, and N′ the second Nicol.   The poles of the horse-shoe, below the bar, are indicated by the dotted circles.   The piece P between the lamp and the first Nicol was often found useful in the more delicate observations : it is a metallic screen, containing a long horizontal slit about $\frac{1}{8}$ of an inch wide.   Sometimes the flat flame L has its edge presented to C ; and then, the piece P being in position, the object seen at C through N′ is a small segment of the flame, sensibly square.   But generally, except when the angle of incidence is near 90°, the width of the flame is presented ; and then the object seen in the mirror is a long horizontal rectangle, uniformly illuminated, strongly outlined above and below, and bringing out small changes of small intensity very delicately.

In the diagram, the axis of the bar A B is produced through the end B next the observer ; the extremity F of the axis thus produced is used afterwards as a point of reference.

In all my later observations, the values of the angle of incidence $\frac{1}{2}$ L C E were assigned beforehand as carefully as possible, but by a method which cannot pretend to great accuracy.   A broad sheet of drawing-paper, which had been cut away at the proper angle through one of its corners, was laid flat on the table, and aligned against the fixed stand of

the magnet and the movable stand of the lamp, each of the stands presenting suitably a long straight edge in contact with the table.

It will be observed that, according to these arrangements, the lines of magnetic force at the point of incidence remain sensibly parallel to the trace of the plane of incidence on the reflecting surface, through all changes of incidence from near grazing to near normal.

3. *Specification of Rotations of the Nicols.*—I shall have occasion repeatedly to speak of right-handed and left-handed rotations of the two Nicols. In the employment of these terms, I shall always view the second Nicol from the point E, and the first Nicol from the point C. By a right-handed rotation of the second Nicol, I mean, therefore, a rotation of the analyzer which is with watch-hands when viewed from E ; and by a left-handed rotation of the first Nicol, I mean a rotation of the polarizer which is against watch-hands when viewed from the point of incidence C.

4. *Specification of Magnetizations of the Reflector.*—In the statement of results, I shall always specify the two magnetizations of the reflector by giving the direction of the magnetizing current, the current being supposed to circulate spirally round the bar A B, out of one coil into the other. These directions of magnetizing currents may of course be conceived and remembered more simply, as the directions of Amperean currents in the magnetized bar. By a right-handed current I shall always understand here, a magnetizing current whose effective direction, round the reflecting bar A B, is with watch-hands when viewed from the point F.

The three points of reference now fixed will be carefully adhered to:—E for movements of the second Nicol or analyzer ; C for movements of the first Nicol or polarizer ; and F for directions of the magnetizing current, or for directions of Amperean currents in the magnetized reflector.

5. *Optical Observations with the Mirror and the two Nicols.*— These belong properly to Optics, and present nothing new ; some of them, also, were described in my former paper ; but they are given here for their bearing on subsequent methods and results, and for the useful practice which they afford in the management of the apparatus. Suppose the pieces placed as in the diagram (2).

Whatever be the angle of incidence, there are only two arrangements of the Nicols which give pure extinction of the reflected image. In one of these arrangements the principal section of the first Nicol is parallel to the plane of incidence, and in the other it is perpendicular.

When the incidence is between grazing and principal, 90° to 85°, and the two Nicols are initially at pure extinction, any very small rotation of the first Nicol may be neutralized perfectly, or very nearly, in the polariscope, by a small rotation of the second Nicol, provided the two rotations are both right-handed or both left-handed, and properly related to each other in magnitude ; but if the two rotations are one right-handed and the other left-handed, the effect of one in the polariscope is strengthened by the other *ab initio*.

When the incidence is between principal and normal, 65° to 30°, and the two Nicols are initially at pure extinction, any very small rotation of the first Nicol may be similarly neutralized by a small rotation of the second Nicol, provided the rotations are one right-handed and the other left-handed ; but if the rotations are both right-handed or both left-handed, the effect of one is strengthened by the other *ab initio*.

When the incidence is about 75°, and the two Nicols are initially at pure extinction, no small rotation of the first Nicol can be neutralized, or its effect in the polariscope sensibly weakened, by a rotation of the second Nicol either way.

It appears from these observations, that the arrangement of the two Nicols for extinction is very sharply defined at or very near the principal incidence only. At other incidences there is a sensible though small range of angular magnitude, increasing as the angle of incidence approaches either $\frac{\pi}{2}$ or 0, through which we can turn the first Nicol either way from pure extinction, without losing the power to recover a pretty good extinction by a proper displacement of the second Nicol.

The next observation requires a compensating slip of glass. That which I generally employ is a piece of good plate, $\frac{1}{8}$ of an inch thick, 1 wide, and 7 or 8 long. It is held between the mirror and the second Nicol, its plate-faces perpendicular to the reflected ray, and its length at 45° to the plane of reflection. If the slip has been properly chosen, and is in good condition, it is quite inactive in the polariscope, except when purposely strained by the observer's hands. The only strains applied are compression along the length and tension along the length.

When the angle of incidence is about 75°, and the two Nicols are initially at pure extinction, any very small rotation of the first Nicol is neutralized perfectly or very nearly by the compensating slip feebly strained : the right-handed rotation is neutralized by compression right hand down, and, therefore, also by tension left hand down ; and the left-handed rotation is neutralized by tension right hand down, and, therefore, also

by compression left hand down. By a contrary strain with the same hand down in each of these cases, or by a similar strain with the other hand down, the effect of rotation of the first Nicol in the polariscope is very clearly strengthened *ab initio*. These observations with the slip of strained glass were described and explained in my former paper. It may be remembered that a right-handed rotation of the first Nicol was there compensated by tension right hand down, and not, as here, by compression right hand down. The reason of this difference is obvious, the plane of reflection being there vertical, but here horizontal. I proceed now to the observations with the magnetized mirror and the two Nicols, placed always as in the diagram (2).

6. *First Experiment.*—The plane of polarization of the light incident upon the mirror is constantly parallel to the plane of incidence ; and the initial extinction is made as pure as possible.

(1) The second Nicol is turned righthandedly through an extremely small angle from the position of extinction ; and the light thus restored faintly in the polariscope is watched for changes of intensity when the reflecting bar is magnetized successively by contrary currents.

(2) The second Nicol is turned lefthandedly through an extremely small angle past the position of extinction ; and the optical effects of contrary magnetizing currents are observed as in the former case. The following is an accurate statement of the results :—

(1) The light restored from extinction by a very small right-handed displacement of the second Nicol is always strengthened by a right-handed magnetizing current, and always weakened by a left-handed current.

(2) The light restored from extinction by a very small left-handed displacement of the second Nicol is always weakened by a right-handed current, and always strengthened by a left-handed current.

The intensity of these optical effects of magnetization varies very noticeably with the angle of incidence. About incidence 85° the effects are very faint, but perfectly regular and much better than merely sensible ; about incidence 75° they are more distinct, and very sensibly stronger ; about incidences 65° and 60° they are comparatively clear and strong, a good deal stronger than at 75° ; about incidence 45° they are still pretty strong, but very sensibly fainter than at 60° ; about incidence 30° they are again very faint, much the same as at 85°.

Between 30° and the normal I have made very few observations, and these not satisfactory ; between 85° and 90° I

have obtained no sure effect, nothing that could be recovered with certainty ; but within the preceding range of incidence, 85° to 30°, I have always recovered the effects easily, and always in the forms now stated.

The right-handed current conspires with (or strengthens the effect of) a right-handed rotation of the second Nicol, and so forward consistently, the optical effect of the current being reversed by reversal of the current, and also by reversal of rotation of the second Nicol.

7. Returning for a moment to the method of observation, suppose the second Nicol turned righthandedly through a small angle from extinction.

(1) Only right-handed currents are applied, the circuit being closed and broken at intervals.   When the circuit is closed, the light is strengthened in the polariscope ; when the circuit is broken, the light is weakened.

(2) Only left-handed currents are applied.   When the circuit is closed, the light is weakened ; when the circuit is broken, the light is strengthened.

(3) Contrary currents are transmitted in succession, the reversal being made by a rapid half-turn of the commutator. In this case, the passage from left-handed current to right-handed strengthens the light in the polariscope, and the contrary passage from right-handed current to left-handed weakens the light.

The magnetic changes of the bar in (3) are more than twice as great as those in (1) or (2), because of the imperfect demagnetization of the bar at the instant of *break*.   It is observed accordingly in the experiment, that the optical changes in (3) are far superior to those in (1) and (2), generally much more than twice as strong.   Sometimes, indeed, about the extreme incidence 85° or 30°, and when circumstances are unfavourable, I find the effects in (3) still quite distinct, while those in (1) and (2) are almost or altogether imperceptible.   Another fact of the same kind which I have noticed in (1) and (2) is, that the effect of *make*, whether it be an increase or a diminution of intensity in the polariscope, is generally more distinct, strikes the eye more sharply, than the effect of *break*.   I may state here finally that, when the effects are very weak, the observer may often bring them out better and better by making the rotation of the second Nicol smaller and smaller till the restoration in the polariscope is merely an imperfect extinction.

8. *Second Experiment.*—The plane of polarization of the light incident on the mirror is constantly perpendicular to the plane of incidence.   All the other arrangements, and the observations, are precisely as in the first experiment.   The

second Nicol is turned through an extremely small angle from the position of extinction, first righthandedly, then left-handedly; and in each case the effects of the two magnetizing currents are observed in the polariscope.

About incidence 85°, the light restored by a right-handed rotation of the second Nicol is strengthened by a right-handed current, and so forward, the effects being undistinguishable in any way from those obtained in the first experiment at the same incidence, except that (under equally favourable conditions) they are certainly and considerably fainter. About incidence 80° the effects are still of the same kind, but a good deal fainter—so faint, indeed, that they cannot be brought out very distinctly except under the most favourable conditions (the battery fresh, the initial extinction very pure, and the displacement of the second Nicol extremely small). About incidence 75° the regular effects disappear. About incidence 70° they reappear very faintly, as faintly as at 80°, but quite distinctly contrary to those obtained at 80° and 85°: the light restored by a right-handed rotation of the second Nicol is now weakened by a right-handed current, and strengthened by a left-handed current, and so forward. At incidences 65°, 60°, 45°, 30°, the effects are of the same kind as at 70°, still contrary, therefore, to those obtained at 85°; about incidence 60° they are comparatively clear and strong, though sensibly fainter than those obtained in the first experiment at the same incidence ; about 30° they are faint but still distinct, and clearly stronger than the contrary effects obtained at 85°. It appears thus that, in the second experiment, the optical effects of magnetization fall under two distinct cases :—

(1) Between grazing and principal incidences, the law is the same as in the first experiment : the right-handed current conspires with a right-handed rotation of the second Nicol, and so forward.

(2) Between principal and normal incidences, this law is simply reversed : the left-handed current conspires with a right-handed rotation of the second Nicol, and so forward.

9. Any details that could be given as to the methods of observation in the second experiment would be a virtual repetition of article (7) ; but some remarks are due to the case of principal incidence. In my earlier observations at and about incidence 75° in this experiment, I was much perplexed with the results. The effects of magnetization were sometimes imperceptible, while in other cases they were quite sensible though faint. Sometimes they were similar to the effects at 85°, and sometimes similar to the contrary effects at 60°; but they were more frequently of another kind : either the right-

handed current or the left-handed, sometimes one and some-
times the other, conspired feebly with each rotation of the
second Nicol, the right-handed and the left-handed in succession.
The latter effects were certainly due to slight misplacements of
the first Nicol, which impaired the purity of the initial extinc-
tion.   All these irregularities disappeared in the most carefully
conducted of my later observations, where due attention was
given to two principal conditions—the assignment of a proper
value (75° exactly or nearly) to the angle of incidence, and
the purification of the initial extinction by exact adjustment
of the first Nicol.   Upon the whole, the result of my observa-
tions is that, in the second experiment, at and about incidence
75°, neither magnetization has any regular effect in the pola-
riscope.   I have little doubt that, with higher powers, magne-
tization would have a distinct optical effect, and that effect an
increase of intensity in every case; but I merely describe
things at present as I have seen them.

10. *Third Experiment.*—The two Nicols are placed initially
at pure extinction, the plane of polarization of the light inci-
dent on the mirror being parallel to the plane of incidence.
The second Nicol remains fixed.

The first Nicol is turned through an extremely small angle
from the position of extinction, first righthandedly and then
lefthandedly; and in each case the effects of the two magne-
tizing currents are observed in the polariscope.

About incidence 85° the light restored by a right-handed
rotation of the first Nicol is weakened by a right-handed cur-
rent, and strengthened by a left-handed current; while the
light restored by a left-handed rotation of the first Nicol is
strengthened by a right-handed current, and weakened by a
left-handed current.   The effects are fainter than those obtained
at the same incidence in the first experiment, but they are
distinct and perfectly regular.   About incidence 80° the
effects are still regular and of the same kind, but very faint,
and requiring extremely small displacements of the first Nicol
to bring them out clearly.   About incidence 75° the regular
effects disappear; and some irregular effects, which make their
appearance here as in the second experiment, are eliminated
by the assignment of a proper value to the angle of incidence,
and by exact adjustment of the second Nicol.

At incidences 65°, 60°, 45°, 30°, the effects are contrary to
those obtained at 85° : the light restored by a right-handed
rotation of the first Nicol from extinction is strengthened by
a right-handed current, and so forward.   At incidence 60°,
and even at 45°, the effects are very distinct and comparatively
strong, but always fainter than those obtained at the same in-

cidences in the first experiment. It appears thus that in the third experiment, as in the second, the optical effects of magnetization fall under two cases :—

(1) Between grazing and principal incidences the right-handed current conspires with a left-handed rotation of the first Nicol, and so forward consistently.

(2) Between principal and normal incidences, the preceding law is simply reversed ; the right-handed current conspires with a right-handed rotation of the first Nicol, and so forward.

11. *Fourth Experiment.*—The two Nicols are placed initially at pure extinction, the plane of polarization of the light incident on the mirror being perpendicular to the plane of incidence. All the other arrangements and the procedure are as in the third experiment : the second Nicol remains fixed ; and the first Nicol is displaced through a very small angle from the position of extinction, first righthandedly and then lefthandedly. The results are very similar to those obtained in the first experiment.

(1) The light restored by a right-handed rotation of the first Nicol is always weakened by a right-handed current, and always strengthened by a left-handed current.

(2) The light restored by a left-handed rotation of the first Nicol is always strengthened by a right-handed current, and always weakened by a left-handed current.

Very near grazing incidence, between 90° and 85°, the effects are insensible ; about incidence 85° they are very faint, but regular and quite distinct ; they increase in strength quite evidently through the incidences 80°, 75°, 70°, to somewhere between 65° and 60°, where they are, I think, as clear and as strong as those obtained in the first experiment ; they then diminish gradually to incidence 30°, where they are very faint, but still somewhat stronger than at 85°.

It appears thus that, in the fourth experiment, the right-handed current conspires always with a left-handed rotation of the first Nicol.

12. The four experiments which have been described were repeated at several incidences with mirrors of steel. Some finely polished knife-blades were tried, and several masses of other forms. The best was a small bar-magnet of hard steel, which had one of its narrow faces polished on a cutler's wheel (one of those large wheels used for sword-blades). The curvature of this mirror was inconsiderable ; and its polish was extremely fine. The arrangements were as in the diagram (2), the bar being laid stably from pole to pole of the horseshoe.

All the old effects were recovered regularly. Only one thing new attracted my notice, which was, the greater superi-

ority of *reversal* to *break*, and even of *make* to *break*, in the case of steel, than in the case of soft iron (7) ; but I do not know how far my judgment in this matter may have been influenced by expectation.

13. *Synopsis of the preceding Results—two Laws with two Exceptions.*

First Law. The right-handed current conspires with a small right-handed rotation of the analyzer from extinction ; and so forward.

Second Law. The right-handed current conspires with a small left-handed rotation of the polarizer from extinction ; and so forward.

First Exception. When the plane of polarization of the light incident on the mirror is perpendicular to the plane of incidence, the First Law is reversed for all incidences between principal and normal.

Second Exception. When the plane of polarization of the light incident on the mirror is parallel to the plane of incidence, the Second Law is reversed for all incidences between principal and normal.

14. I may say here, with reference to the various statements of fact which I have made in the preceding articles, that they are founded on a large number of perfectly concordant observations. The angles of incidence quoted are of course only rough approximations (2); and some of the other details may be modified by future observation; but the broad facts are as certain to my mind as any thing in Physics.

The next two experiments afford interesting verifications of former results, as they show that when two actions already known as conspiring actions are applied separately, their optical effects are similar, or rather similarly directed. To prepare the way, I must mention some delicate optical phenomena, which present themselves in connexion with very small movements of either Nicol through the position of extinction. Each Nicol is supposed to be near the mirror ; the piece P is in position (2); and the incidence is between principal and normal. When the extinction is perfect, and the observer's attention has been once directed, he can generally detect an obscure cloud, which is pretty definitely formed, and sometimes finely formed as a dark horizontal band, covering the old place of the reflected image, and extending well across the field. In these circumstances, any almost immeasurably small rotation of either Nicol, in one direction or the contrary, produces a regular and very sensible change in the field of vision—a continuous displacement of the band, upwards in one case and downwards in the other, the form and direction of

the band being fairly preserved through large displacements. In the next two experiments, I make use of this movement of the band as a mere initiation of restoration from pure extinction in the polariscope. I call it a mere initiation of restoration, because the band has already undergone a large displacement over the surface of the mirror, by rotation of the second Nicol or otherwise, before there is any sensible restoration of the re- flected image from pure extinction.

15. *Fifth Experiment.*—The plane of polarization of the light incident on the mirror is parallel to the plane of inci- dence; and the extinction in the polariscope is made as pure as possible, the old place of the reflected image being covered by an obscure cloud or band (art. 14). The observations now to be described in this article and in art. 16 have been made re- peatedly at the four incidences 70°, 65°, 60°, 45°, with con- sistent and uniformly distinct results.

(1) The two Nicols are untouched, and remain in their initial positions of pure extinction. A left-handed current sends the band up through a small distance very clearly ; a right-handed current sends the band down ; contrary currents in rapid succession act accordingly, and give larger displace- ments of the band (art. 7).

(2) With open circuit, a right-handed rotation of the second Nicol through the position of extinction sends the band down very clearly. Compared with observation (1), this verifies the First Law stated in art. 13, that the right-handed current con- spires with a right-handed rotation of the second Nicol.

(3) With open circuit, a right-handed rotation of the first Nicol through the position of extinction sends the band down certainly, though the effect is not generally so good as that in (2). The results in (1) and (3) verify the exception to the second Law stated in art. 13, as the present incidences lie be- tween principal and normal.

16. *Sixth Experiment.*—The plane of polarization of the light incident on the mirror is perpendicular to the plane of incidence ; other things as in art. 15.

(1) The effects of magnetization obtained in art. 15 are simply reversed. A right-handed current sends the band up very clearly ; a left-handed current sends the band down; con- trary currents in rapid succession act accordingly.

(2) With open circuit, a right-handed rotation of the second Nicol sends the band down (exactly as in art. 15). This agrees with the exception to the First Law stated in art. 13.

(3) With open circuit, a right-handed rotation of the first Nicol sends the band down (exactly as in art. 15). This agrees with the Second Law (art. 13).

The reader would have a false view of these two experiments, if he thought there was any thing uncertain about the phenomena, any mere guess-work in the observations. On the contrary, the effects are perfectly regular, almost invariably very sensible, and sometimes beautifully distinct. In one or two trials which I conducted in favourable circumstances along with several friends, I found that the movements of the band, whether produced by currents or by displacements of the second Nicol, were sufficient to make a strong, correct, and immediate impression upon an eye quite uneducated, and which had been merely directed to the right point.

I think that any discussion of the optical phenomena here utilized would be quite irrelevant.

17. In the next two experiments, I apply the compensating slip of glass already described (art. 5). It is introduced into the path of the reflected light, between the mirror and the second Nicol, its length at 45° to the plane of reflection, and its plate-faces perpendicular to the ray. Of the four methods of straining the glass, I generally choose *compression right hand down*. To apply the action, I hold the slip at the lower end, between finger and thumb of the right hand, taking care to have the piece properly directed, and exert a small pressure downwards by the fore-finger of the left hand laid along the upper end of the slip. A very feeble pressure is sufficient to give a perceptible restoration of the reflected image from pure extinction.

18. *Seventh Experiment.*—The plane of polarization of the light incident on the mirror is parallel to the plane of incidence; and the two Nicols are kept at pure extinction.

When the light is restored very faintly by the compressed glass, it is not sensibly strengthened or weakened by either magnetization of the mirror; and this holds true for all incidences between 85° and 30°. Sometimes, indeed, when working in this way, I obtained sure though very faint changes of intensity in the polariscope—an increase by one current, and a decrease by the other; but in several cases where these effects were closely examined, they were found to be caused by imperfection of initial extinction, and particularly by slight misplacements of the second Nicol. Upon the whole, if there was any regular effect here, it was too faint to be certainly characterized.

19. *Eighth Experiment.*—The plane of polarization of the light incident on the mirror is perpendicular to the plane of incidence; and the initial extinction is made as pure as possible, particular attention being given to the placing of the first Nicol. Other things are as in the seventh experiment.

About incidence 75°, the light restored by compression right hand down is strengthened by the right-handed current, and weakened by the left-handed current, the actions of the currents from reversal being regular and very clear though faint.

About incidence 60° the effects are of the same kind, but a great deal stronger. Each of the currents acts clearly now from its own break (art. 7), the right-handed current conspiring always with compression right hand down, therefore also with tension left hand down ; and the left-handed current conspiring always with tension right hand down, therefore also with compression left hand down. These four cases of conspiring actions, as well as the four corresponding cases of contrary or mutually compensating actions (right-handed current with tension right hand down, &c), are all brought out regularly and strongly about incidence 60°.

At incidence 45° the effects are still of the same kind, regular and very distinct though not strong, not nearly so strong as at 60°. Between principal incidence and grazing, about 85°, the effects are still of the same kind, the right-handed current clearly conspiring here as elsewhere, though here very faintly, with compression right hand down.

20. It is worth noticing that we might have anticipated some of the results brought out in the last two experiments, those of them, namely, that are obtained at incidences near 75°. It has been mentioned already as a result easily obtained with the present apparatus, that a small right-handed rotation of the first Nicol from extinction, at or near incidence 75°, is neutralized by compression right hand down (art. 5). From this we infer that compression right hand down is optically equivalent, at least in an approximate manner, to a left-handed rotation of the first Nicol. We have, then, two cases, according to the fourth and third experiments :—

(1) When the plane of polarization of the light incident on the mirror is perpendicular to the plane of incidence, the right-handed current conspires with a left-handed rotation of the first Nicol, and should conspire therefore with the optical equivalent of that rotation, compression right hand down, as it does actually in the eighth experiment.

(2) When the plane of polarization is parallel to the plane of incidence, and the angle of incidence is about 75°, neither current conspires with any small rotation of the first Nicol ; so that neither current should conspire with compression right hand down, which is actually the case in the seventh experiment.

21. I have now given all my positive results ; but there are

two other lines of experiment which I have tried without effect, and which ought to be briefly noticed.

(1) The mirror, as formerly, an equatorial face of a magnetized bar, the plane of incidence perpendicular to the lines of magnetic force, and the incidence varying from near normal to near grazing. The arrangements were of course somewhat different from those already described ; but they were not more difficult, and were made with equal care ; and I think that the experiments were altogether as delicate as any of the preceding. Working in this way at different times, I saw no appearance of optical effect of magnetization.

(2) The mirror an equatorial face of a magnet, the incidence normal, and the inclination of the plane of incidence to the lines of magnetic force varying from $0°$ to $90°$. As the normal incidence was obtained by the employment of a mirror of unsilvered glass, the light was a good deal weaker than formerly ; but otherwise the experiment was as delicate as any of the preceding. Nothing like an optical effect of magnetization was observed in any instance.

From these experiments, and from all that I have seen upon the subject, I think it probable, in the highest degree, that magnetization of a reflector even to saturation would be absolutely without optical effect in the cases now exemplified (that is, in the case of normal incidence upon an equatorial face), and in any case where the fronts of the incident and reflected waves are parallel to the lines of magnetic force.

I return now to the consideration of our first arrangement, where the lines of magnetic force are parallel to the intersection of the reflecting surface and the plane of incidence.

22. Law of the optical action of magnetism at incidences near grazing.

Whatever be the angle of incidence between grazing and principal, the effect of magnetization of the mirror, when sensible, is to turn the plane of polarization of the reflected light through a very small angle, in a direction always contrary to that of the Amperean currents ; for, whatever be the angle of incidence between grazing and principal, the two laws stated in art. 13 hold true throughout the first four experiments without exception.

(1) The right-handed current conspires with a right-handed rotation of the second Nicol. But the effect of a right-handed rotation of the second Nicol (before magnetization of the mirror) is virtually to turn the reflected ray lefthandedly, or to displace its plane of polarization lefthandedly, with reference to the principal section of the analyzer. And since the right-handed current conspires with the right-handed rotation of the

analyzer, it adds actually to the virtual left-handed rotation of the reflected ray, or turns the plane of polarization left-handedly.

(2) The right-handed current conspires with a left-handed rotation of the first Nicol.   But we have seen in art. 5, that a left-handed rotation of the first Nicol is neutralized (near grazing incidence) by a left-handed rotation of the second Nicol ; and from this we infer that a left-handed rotation of the first Nicol turns the plane of polarization of the reflected ray lefthandedly.   And since the right-handed current conspires with a left-handed rotation of the first Nicol, it adds to the consequent left-handed rotation of the reflected ray, or turns the plane of polarization lefthandedly.

Of course this proof assumes, and the conclusion implies, that the reflected light may be considered as approximately plane-polarized in all the preceding instances of conspiring actions, as well as in the corresponding instances of mutually compensating actions, both in the optical observations (art. 5), and throughout the first four experiments.

23. It is proved thus beyond question, at least as a very approximate expression of facts, that, near grazing incidence, the effect of magnetization of the mirror upon a reflected ray is to turn the plane of polarization through an extremely small angle, in a direction contrary to that of the Amperean currents. Under what conditions, on what assumptions, may this be accepted as the law of the action at all incidences ?   To prepare for a definite answer to this question, I shall first subject the statement of the law to a simple transformation.

When the vibration reflected from the unmagnetized mirror is either parallel or perpendicular to the plane of reflection, the effect of magnetization is to introduce a new and very small component vibration in a direction perpendicular to the primitive vibration, the sense of the new component being that assumed by the primitive vibration when turned through a right angle in a direction contrary to the Amperean currents. And for incidences between grazing and principal, the difference of phases of the two components (the primitive and the new) is much nearer to 0 than to $\frac{\pi}{2}$.

It is important to observe here, that the results obtained in the fifth and sixth experiments necessitate the assumption of some such law as this, even for incidences between principal and normal.   For the direction of the primitive vibration is in those experiments exactly parallel or perpendicular to the plane of reflection, and the Nicols remain constantly in their initial position of pure extinction ; so that the observed effects

of magnetization, effects which are of the same kind as those produced by rotation of the second Nicol, cannot be explained by any mere changes of the primitive vibration in amplitude or phase, or by any thing except the introduction of a new and very small component in a direction perpendicular to the primitive vibration.

24. *General Law of the Action of Magnetism upon the Reflected Ray.*

The three following assumptions appear to me to afford a perfect explanation of all the principal phenomena.   They were suggested as above, and were tested by a careful mathematical discussion of the results of all the experiments in succession.   The discussion presents little difficulty, but is too tedious to be offered here.

(1) When the original vibration is parallel or perpendicular to the plane of reflection, the effect of magnetization of the mirror is to turn the vibration through a very small angle in a direction contrary to the Amperean currents.

The resolved parts of the vibration so turned, one in the direction of the primitive vibration and the other perpendicular to it, may be called the primitive component and the new component respectively, as in art. 23.

(2) The primitive component is always reflected according to the same laws of retardation of phase, after magnetization of the reflector as before.

(3) Whether the new component be parallel or perpendicular to the plane of reflection, and whatever be the angle of incidence, the phase-retardation of the new component (with reference to a standard reflected ray, polarized in the plane of incidence, and incident in the same phase as the actual primitive), is always an angle in the first quadrant, and much nearer to zero than to $\frac{\pi}{2}$.

It will be admitted that the assumption (3) is a very remarkable one, and very important if true.   I hope to see this geometric theory of the phenomena verified by the mathematicians, or something better put in its place.

25. It would be superfluous now to offer any explanation of the absence of all optical effect of magnetization in the case of normal incidence (art. 21).   It is not so easy to understand the absence of effect at incidences very near grazing, 85° to 90°, in the first four experiments.   We might expect indeed, on the contrary, that as the ray approaches parallelism to the lines of force, or as the front of the wave approaches perpendicularity, the magnetic force would act at a greater advantage, and the optical effect would therefore become stronger.

But against this we have what appears to be fairly established by observation as a general truth in optics, that the specific differences of reflectors become less and less marked as incidence approaches grazing, until at grazing they almost entirely disappear.

26. I shall not make any lengthened comparison between the effects now observed in connexion with the equatorial surface and those formerly obtained in the case of the polar surface. The two sets of results are not inconsistent, nor do they differ materially in any way, except that the present set contains a larger amount of detail as to the variations of optical effect, variations extending even to reversal. I have no doubt that this enlargement of results is due to better means and improved arrangements in the present series of experiments. It will be remembered that a submagnet, separated from the mirror by a very narrow chink, was found indispensable in the case of the polar surface. In the present experiments there is no place for such an adjunct, and the reflector is fully exposed to view at all incidences, which is a great improvement. The new arrangements are also simpler and more manageable upon the whole, and much better adapted for delicate and exact optical observation.

In one respect I have certainly found the polar surface superior to the equatorial. In the case of the polar surface, and with the power that I have applied, it is easy to obtain, by magnetic force alone, a very distinct restoration of the reflected light from pure extinction, though the restoration is never very strong; but in the case of the equatorial surface, and with equal or greater powers, I have never seen any stronger effect of unassisted magnetic force than those fine movements of the band in the fifth and sixth experiments.

27. The first facts of magneto-optics discovered long ago by Faraday, their more immediate consequences discovered afterwards by Verdet and others—these and the additional facts now published by myself must be all included ultimately under one physical theory. It is very probable that the remarkable theory of magnetism which has been advanced by Sir William Thomson in a discussion of the former class of facts, will apply as well to the latter. Probably also the theory itself may receive additional confirmation in the process. I think it equally probable that the new facts will find important applications in the mechanical parts of the Wave Theory. But in any event there is a new physical action secured thoroughly to science, a specific action of magnetized iron upon light incident at any point of its surface.

Glasgow, January 21, 1878.

XXV. *Proof of the hitherto undemonstrated Fundamental Theorem of Invariants.* By J. J. SYLVESTER, *Professor of Mathematics at the Johns Hopkins University, Baltimore*[*].

I AM about to demonstrate a theorem which has been wait-ing proof for the last quarter of a century and upwards. It is the more necessary that this should be done, because the theorem has been supposed to lead to false conclusions, and its correctness has consequently been impugned[†]. But, of the two suppositions that might be made to account for the observed discrepancy between the supposed consequences of the theorem and ascertained facts—one that the theorem is false and the reasoning applied to it correct, the other that the theorem is true but that an error was committed in drawing certain de-ductions from it (to which one might add a third, of the theorem and the reasoning upon it being both erroneous)— the wrong alternative was chosen. An error was committed in reasoning out certain supposed consequences of the theorem ; but the theorem itself is perfectly true, as I shall show by an argument so irrefragable that it must be considered for ever hereafter safe from all doubt or cavil. It lies at the basis of the investigations begun by Professor Cayley in his ' Second Memoir on Quantics,' which it has fallen to my lot, with no small labour and contention of mind, to lead to a happy issue,

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[*] Communicated by the Author.

[†] Thus in Professor Faà de Bruno's valuable *Théorie des Formes Binaires*, Turin, 1876, at the foot of page 150 occurs the following pas-sage:—" Cela suppose essentiellement que les équations de condition *soient toutes indépendantes entr'elles, ce qui n'est pas toujours le cas*, ainsi qu'il résulte des recherches du Prof. Gordan sur les nombres des cova-riants des formes quintique et sextique."

The reader is cautioned against supposing that the consequence alleged above does result from Gordan's researches, which are indubitably correct. This supposed consequence must have arisen from a misapprehension on the part of M. de Bruno of the nature of Professor Cayley's rectification of the error of reasoning contained in his second memoir on Quantics, which had led to results discordant with Gordan's. Thus error breeds error, unless and until the pernicious brood is stamped out for good and all under the iron heel of rigid demonstration. In the early part of this year Mr. Halsted, a Fellow of Johns Hopkins University, called my attention to this passage in M. de Bruno's book ; and all I could say in reply was that " the extrinsic evidence in support of the independence of the equa-tions which had been impugned rendered it in my mind as certain as any fact in nature could be, but that to reduce it to an exact demonstration transcended, I thought, the powers of the human understanding."

At the moment of completing a memoir, to appear in Borchardt's Journal, demonstrating my quarter-of-a-century-old theorem for enabling Invariants to procreate their species, as well by an act of self-fertilization as by conjugation of arbitrarily paired forms, the unhoped and unsought-for prize fell into my lap, and I accomplished with scarcely an effort a task which I had believed lay outside the range of human power.

and thereby to advance the standards of the Science of Algebraical Forms to the most advanced point that has hitherto been reached. The stone that was rejected by the builders has become the chief corner-stone of the building.

I shall for greater clearness begin with the case of a single binary quantic $(a, b, c, \ldots, l \,\mathcal{)}\, x, y)^i$. Any rational integral function of the *elements* $a, b, c, \ldots l$ which remains unchanged in value when for them are substituted the elements of the new quantic obtained by putting $x + hy$ instead of x in the original one, I call a Differentiant in x to the given quantic.

By a differentiant of a given weight w and order j, I mean one in every term of which the combination of the elements is of the jth order and the sum of their weights w, the weights of the successive elements $(a, b, c, \ldots l)$ themselves being reckoned as $0, 1, 2, \ldots i$ respectively.

The proposition to be proved is, that the number of arbitrary constants in the most general expression for such differentiant is the difference between the number of ways in which w can be made up with the integers $0, 1, 2, 3, \ldots i$ (repetitions allowable), less the number of ways in which $w-1$ can be made up with the same integers. We may denote these two numbers by $(w : i, j)$, $(\overline{w-1} : i, j)$ respectively, and their difference by $\Delta(w : i, j)$. Then, if we call the number of arbitrary constants in the differentiant of weight w and order j belonging to a binary quantic of the ith order $\mathrm{D}(w : i, j)$, the proposition to be established is that $\mathrm{D}(w : i, j) = \Delta(w : i, j)$.

Let us use Ω to denote the operator

$$a\frac{d}{db} + 2b\frac{d}{dc} + \ldots + ik\frac{d}{dl},$$

and O to denote the operator

$$ib\frac{d}{da} + (i-1)c\frac{d}{db} + \ldots + l\frac{d}{dk}.$$

Then it is well known that the necessary and sufficient condition for D being a differentiant in x is that the *identity* $\Omega\mathrm{D} = 0$ be satisfied.

Let us study the relations of Ω and O in respect to D.

In the first place, let U be any rational integral function of the elements of order j and weight w; then I say that

$$\Omega \,.\, \mathrm{O} \,.\, \mathrm{U} - \mathrm{O} \,.\, \Omega \,.\, \mathrm{U} = (ij - 2w)\mathrm{U}.$$

For if we use $*$ to signify the act of pure differential operation, it is obvious that

$$\Omega \,.\, \mathrm{O} \,.\, \mathrm{U} = (\Omega \times \mathrm{O})\mathrm{U} + (\Omega * \mathrm{O})\mathrm{U},$$
$$\mathrm{O} \,.\, \Omega \,.\, \mathrm{U} = (\Omega \times \mathrm{O})\mathrm{U} + (\mathrm{O}*)\mathrm{U};$$

$$\therefore \Omega.O.U - O\Omega.U = \big((\Omega*O)-(O*\Omega)\big)U$$

$$= ia\frac{d}{da} + 2(i-1)b\frac{d}{db} + 3(i-2)c\frac{d}{dc} + \ldots + ik\frac{d}{dk}$$

$$-ib\frac{d}{db} - 2(i-1)c\frac{d}{dc} - \ldots - 2(i-1)k\frac{d}{dk} - il\frac{d}{dl}$$

$$= ia\frac{d}{da} + (i-2)b\frac{d}{db} + (i-4)c\frac{d}{dc} - \ldots - (i-2)k\frac{d}{dk} - 2l\frac{d}{dl}.$$

If now $\rho a^p.b^q.c^r\ldots l^t$, where ρ is a number, be any term in $.$ U, we have

$$\left.\begin{array}{c} p+q+r+\ldots+t=j \\ q+2r+\ldots+it=w \end{array}\right\} \text{ by hypothesis;}$$

$$\therefore \Omega.O.U - O.\Omega.U,$$

i. e.

$$i\left(a\frac{d}{da} + b\frac{d}{db} + c\frac{d}{dc}\ldots + l\frac{d}{dl}\right)$$

$$-2\left(\qquad b\frac{d}{db} + 2c\frac{d}{dc}\ldots + il\frac{d}{dl}\right)$$

$$= \Sigma\rho(ij-2w)(a^p.b^q.c^r\ldots l^t)$$

$$= (ij-2w)U, \text{ as was to be proved.}$$

If now for U we write D a differentiant in x, we have $\Omega D = 0$, and therefore

$$\Omega.O.D = \delta D,$$

where $\delta = if - 2w$.

Again,

$$\Omega.O(O.D) - O.\Omega(O.D) = \big(ij - 2(w+1)\big)O.D;$$

for O.D is of the weight $w+1$;

$$\therefore \Omega^2.O^2.D = \Omega.O\,\delta D + (\delta-2)\Omega.O.D$$

$$= (2\delta-2)\Omega.O.D$$

$$= \delta(2\delta-2)D.$$

Similarly it will be seen that

$$\Omega^3.O^3.D = \delta(2\delta-2)(3\delta-6)D,$$

and in general

$$\Omega^2.O^2.D = \delta(2\delta-2)(3\delta-6)\ldots\big(q\delta-(q^2+q)\big)D$$

$$= (1.2.3\ldots q)(\delta.\overline{\delta-1}\,\overline{\delta-2}\cdots\overline{\delta-q-1})D,$$

the successive numbers δ, $2\delta-2$, $3\delta-6$, &c. being the successive sums of the arithmetical series δ, $\delta-2$, $\delta-4$, $\delta-6$, &c.

To find the most general differentiant in question, we must take every combination of the elements whose weight is w and order j, of which the number is obviously $(w : i, j)$, and prefix an indeterminate constant to each such combination; then operating upon this form with Ω, we shall reduce its weight by unity, and shall obtain as many combinations of this reduced weight (the order j remaining unchanged) as there are units in $(w-1 : i, j)$. Each of these combinations will have for its coefficient a linear function of the assumed indeterminate coefficients; and in order to satisfy the identity $\Omega D = 0$, each such linear function must be made equal to zero. There are therefore $(w : i, j)$ quantities connected by $(w-1 : i, j)$ homogeneous equations. *Supposing the equations to be independent,* the number of the indeterminate coefficients left arbitrary is obviously the difference between these quantities, viz. $\Delta(w : i, j)$. The difficulty consists in proving this independence—a difficulty so great that I think any one attempting to establish the theorem, as it were by direct assault, in this fashion, would find that he had another Plevna on his hands. But a position that cannot be taken by storm or by sap may be *turned* or starved into surrender; and this is how we shall take our Plevna. Be the equations of condition linearly independent or not, it is obvious that we must have $D(w : i, j)$ equal to or greater than $\Delta(w : i, j)$. I shall show by aid of a construction drawn from the resources of the "Imaginative Reason," and founded on the reciprocal properties that have just been exhibited by the famous O and Ω, that this latter supposition, of the first member of the equation being greater than the second, is inadmissible and must be rejected. Observe that $(0 : i, j)$, the number of ways of making up 0 with j combinations of $0, 1, 2, \ldots i$, is 1; also that $D(0 : i, j)$, the number of arbitrary constants in the most general differentiant in x to the quantic $(a, b, c, \ldots \big(x, y)^i$ of order j and weight 0 is also 1; for such differentiant is obviously λa^n.

Thus we have for all values of w,

$$D(w : i, j) = \text{ or } > (w : i, j) - (w-1 : i, j),$$

and also

$$D(0 : i, j) = (0 : i, j);$$

$$\therefore \ D(w : i, j) + D(w-1 : i, j) + D(w-2 : i, j) + \ldots$$
$$+ D(0 : i, j) = \text{ or } > (w : i, j).$$

If in the above condition, for any assumed value of w, $>$ is the sign to be employed, then the equation $D(w : i, j) = \Delta(w : i, j)$ cannot be satisfied for all values of w. If, on the other hand,

$>$ is not the sign to be employed, then this equation, for *every value of w*, commencing with the assumed one down to 0, must be satisfied. The greatest value of w for given values of i, j, it is well known, is $\frac{ij}{2}$ for ij even, and $\frac{ij-1}{2}$ for ij odd. Let us give to w this maximum value in the above "greater or equal" relation; for brevity, denote the differentiants whose types are $[w, i, j]$, $[\overline{w-1}, i, j] \ldots$ by $[w]$, $[w-1]$, $[w-2]$, &c. respectively, i and j being regarded as constants. It will be convenient to substitute for the number of arbitrary constants in any of these differentiants the same number of linearly independent specific values of them; so that we shall have $D(w : i, j)$ of linearly independent $[w]$'s, $D(\overline{w-1} : i, j)$ of linearly independent $[w-1]$'s, and so on. Now, instead of the $D(\overline{w-q} : i, j)$ differentiants $[w-q]$, let us substitute the same number of the derived forms $(O^q[w-q])$'s. I shall prove that the quantities (*all of the same weight w*) thus obtained are linearly independent of one another.

For (1) suppose that those belonging to any one set $O^q . [w-q]$ are not independent, but are connected by a linear equation. Then, operating upon this equation with $\Omega^q .$, we shall obtain a linear equation between the quantities $[w-q]$ for each quantity $(\Omega^q . O^q . [w-q]$ being a numerical multiple of $[w-q])$, which is contrary to the hypothesis. Again, let there be a linear equation between the quantities contained in any number of sets of the form $O^q . [w-q]$ for which m is the greatest value of q. Then, operating upon this with Ω^m, it is clear that all the quantities in the sets for which $q < m$ will introduce quantities of the form $\Omega^{m-q}[w-q]$ where $m-q > 0$, and which consequently vanish. There will be left, therefore, only quantities of the form $[w-q]$, between which a linear equation would exist, contrary to hypothesis, as in the preceding case. Therefore all the quantities in all the sets are linearly independent. But these are all of the weight w, *i. e.* $\left[\frac{ij}{2} \text{ or } \frac{ij-1}{2}\right]$, and are therefore linear functions of the number of ways in which the integers $0, 1, 2, 3, \ldots i$ can be combined i and j together so as to give the weight w. Therefore being linearly independent, as just proved, their number cannot exceed this last-named number, *i. e.* cannot exceed $(w : i, j)$. That is to say,

$$D(w : i, j) + D(\overline{w-1} : i, j) + \ldots + D(O : i, j)$$

cannot exceed $(w : i, j)$. Therefore every one of the equations

$D(w : i, j) = \Delta(w : i, j)$ must be satisfied from the maximum value of w down to the value 0, which proves *the great hitherto undemonstrated fundamental theorem* for a single quantic.

For any number of quantics the demonstration is precisely similar at all points : there will be as many systems of i, j as there are quantics. $(w : i, j : i', j' : \&c.)$ will denote the number of ways of making up w with j of the integers $0, 1, 2, \ldots i$, with j' of the integers $0, 1, 2, \ldots i'$, and so on. The theorem to be demonstrated will be

$$D(w : i, j : i', j' : \ldots) = \Delta(w : i, j : i' : j' \ldots).$$

$$\Omega \text{ will become } \Sigma \left(a \frac{d}{db} + 2b \frac{d}{dc} + \ldots \right),$$

$$O \quad ,, \quad ,, \quad \Sigma \left(ib \frac{d}{da} + (i-1)c \frac{d}{db} + \ldots \right).$$

It will still be true that $\Omega^q. O^q. D$, where D is a differentiant in x (*i. e.* a function of the elements in all the given quantics which withstand change when these are transformed by writing $x + hy$ for x), is a numerical multiple of D; and D will be subject to the identity $\Omega D = 0$. We shall still have

$$D(w : i, j : i', j' : \ldots) = \text{ or } > \Delta(w : i, j : i', j' : \ldots),$$

and

$$D(0 : i, j : i', j' : \ldots) = (0 : i, j : i', j' : \ldots),$$

and shall be able in precisely the same way as before to demonstrate the impossibility of $\Sigma_{k=w}^{k=0} D(w-k : i, j : i', j')$ being *greater* than $(w : i, j : i', j' : \ldots)$, and so shall be able to infer by the same logical scheme $\Delta(w : i, j : i', j' : \ldots) = D(w : i, j : i', j')$ my extension of Professor Cayley's theorem, which leads direct to the Generating Fractions given in my recent papers in the *Comptes Rendus.*

In a series of articles which I hope to publish in the American Journal of Pure and Applied Mathematics, I propose to give a systematic development of the Calculus of Invariants, taking a differentiant as the primordial germ or unit. I have spoken of a differentiant in x, and of course might have done so equally of a differentiant in y. If we call the former D_x, it is capable of being shown, from the very natures of the forms O and Ω, that if the quantity $ij - 2w$, which may be called the *degree* of D_x, be called δ, then $O^\delta D_x$ becomes a differentiant in y. These may be termed simple differentiants ; but the principle of continuity forbids that we should omit to comprise in the same scheme the intermediate forms $O^q D_x$ or $\Omega^q D_y$, through which simple differentiants in x and y pass into each other. These may be termed mixed differentiants ;

$O^p D_x$ may be termed a differentiant p removed (as we speak of *cousins* once, twice, &c. removed) from x, which will be the same thing as $O^q D_y$ (a differentiant q removed from y) if $p + q$ is equal to the degree, viz. $ij - 2w$. Now all these differentiants, whether simple or mixed, possess a wonderful property, which may be deduced by means of Salmon's Theorem, given in the Philosophical Magazine for August 1877. They are all, in an enlarged sense of the term, Invariants—in this sense to wit, that if the elements are made to undergo a substitution consequent upon or, as we may say, induced by a general linear substitution impressed on the variables, which for greater simplicity of enunciation may be supposed to have unity for the determinant of its matrix, then every differentiant, whether single or double (the latter being equivalent to an invariant), and whether simple or mixed, will remain a Constant Function of the Coefficients of the impressed substitution. To wit, if the differentiant be p removes from x and q removes from y (so that its degree is $p + q$), and if the impressed substitution be $lx + \lambda y$ for x, and $mx + \mu y$ for y, where $l\mu - \lambda m = 1$, then will the differentiant be a constant bipartite quantic in the two sets of coefficients l, m and λ, μ, of the degree q in the former and p in the latter—a theorem which amounts almost to a revolution in the whole sphere of thought about Invariants. I have borrowed the term " Imaginative Reason " from a recent paper of Mr. Pater on Giorgione, in which, as in many of those of Mr. Symonds (I will instance one on Milton in particular), I find a continued echo of my own ideas, and in the latter many of the very formulæ contained in my ' Laws of Verse,' where versification in sport has been made æsthetic in earnest. Surely the claim of Mathematics (its "*Anders-streben*") to take a place among the liberal arts must be now admitted as fully made good. Whether we look to the advances made in modern geometry, in modern integral calculus, or in modern algebra, in each of these a free handling of the material employed is now possible, and an almost unlimited scope left to the regulated play of the fancy. It seems to me that the whole of æsthetic (so far as at present revealed) may be regarded as a scheme having four centres, which may be treated as the four apices of a tetrahedron, viz. Epic, Music, Plastic, and Mathematic. There will be found to be a *common* plane to every three of these, *outside* of which lies the fourth; and through every two may be drawn a common axis *opposite* to the axis passing through the remaining two.

So far is certain and demonstrable. I think it also possible that there is a centre of gravity to each set of three, and that the lines joining each such centre with the outside apex will

intersect in a common point the centre of gravity of the whole body of æsthetic ; but what that centre is or must be I have not had time to think out.

Johns Hopkins University, Baltimore,
November 13, 1877.

Postscript.—In the first fervour of a new conception, I fear that in the manuscript which is now on its way to England I may have expressed myself with some want of clearness or precision on the subject of pure and mixed differentiants. I will therefore add a few more explanatory and vaticinatory words on this subject, through the medium of which I catch a glimpse of the possibility of obtaining a simple proof of Gordan's theorem, just as through the medium of pure differentiants taken *per se* I caught a glimpse (almost immediately afterwards to be converted into a certainty) of the proof of Cayley's theorem given in this memoir. I conceive that what the *ensemble* of pure differentiants have done for the one, the larger *ensemble* of all sorts of differentiants, pure and mixed, taken together, will enable me or some one else to accomplish for the other.

Any function of the coefficients of a quantic which is nullified by the operation upon it of Ω, which we may call the *revector* symbol, or in other words, whose first *revect* is zero, is a pure differentiant in x. So, of course, if nullified by the operation upon it of O, which may be called the *provector* symbol, it is a pure differential in y. We may call $ij - 2w$, where i is the degree of the quantic, j the order of a pure differentiant, and w its weight in x, the grade of the differentiant, and denote this grade by δ.

The δth provect of a pure differentiant in x is of course a pure differentiant in y, which is δ *removes* from x, as the pure differentiant in x is δ removes from y. If q be less than δ, the qth provect of a pure differentiant in x is a mixed differentiant q removes from x, or, if we like to say so, $(\delta - q)$ removes from y. The *grade* of a mixed differentiant may be defined to be the same as that of the pure differentiant of which it is a provect or revect.

Then, in the first place, we have this proposition :—If any linear substitution whatever be impressed in the variables of a quantic, the transformed value of any of its differentiants will separate into two factors, of which one will be the determinant of substitution raised to the power w, where w is the weight corresponding to the order and grade of the differentiant and the degree of the quantic. The remaining factor will be a function of the coefficients of substitution, and may

be called the outstanding factor. Of this I shall proceed to speak.

Let x be replaced by $hx + ly$,

y „ „ $kx + my$.

Then the outstanding factor for the transformed D (a pure differentiant in x of the grade δ) may be proved by repeated applications of Salmon's theorem to be equal to

$$\left(1 + \frac{k}{m}\,\mathrm{O}\,. + \left(\frac{k}{m}\right)^2 \frac{\mathrm{O}^2.}{1 \cdot 2} + \dots\right) m^\delta \mathrm{D},$$

where of course the series of terms in the development will, after the $(\delta + 1)$th term, vanish spontaneously. In other words, the outstanding factor of the transformed D is $m^\delta e^{\frac{\mathrm{KO}.}{m}}.\mathrm{D}$, where it will be noticed that only the coefficients of substitution due to the change in y make their appearance.

If now we take any mixed differentiant, say the qth provect of D, *i. e.* $\mathrm{O}^q.\,\mathrm{D}$, its outstanding factor, I find, will be the qth emanant of the outstanding factor for D, *i. e.* will be

$$\left(h\frac{d}{dk} + l\frac{d}{dm}\right)^q \left(m^\delta e^{\frac{\mathrm{KO}.}{m}}\right)\mathrm{D}.$$

And here for the present I end. The subject is, as it was, a vast one; and this conception of mixed differentiants opens out still vaster horizons. Every thing grown on American soil or born under the influence of its skies, as its lakes, its rivers, its trees, and its political system, seems to have a tendency to rise to colossal proportions.

I will merely add one remark which has occurred to me relating to Sturm's theorem and the process of Algebraical common measure in general. If $f(x, y)$ be a rational integral function of x, y, and $f'(x, y)$ its derivative in respect to x, and we perform the process of common measure between them regarded as functions of x, we know that the irreducible part of the successive remainders taken in ascending order, say $\mathrm{U}_0, \mathrm{U}_1, \mathrm{U}_2, \dots$, will have for their leading coefficients (say $\mathrm{D}_0, \mathrm{D}_1, \mathrm{D}_2 \dots$), the discriminants of f and of its successive derivatives in respect to x respectively.

Here D_0 is an invariant of the given form;

D_1 (a differentiant in x) will be the leading coefficient of the covariant

$$\mathrm{D}_1 x^2 + \mathrm{O}\,.\,\mathrm{D}_1 xy + \frac{\mathrm{O}^2.}{1 \cdot 2}\,\mathrm{D}_1 y^2;$$

D₂ (another differentiant in x) will be the leading co-efficient of the covariant

$$D_2 x^4 + O D_2 x^3 y + \frac{O^2.}{1 \cdot 2} D_2 x^2 y^2 + \frac{O^3.}{1 \cdot 2 \cdot 3} D_2 x y^3 + \frac{O^4.}{1 \cdot 2 \cdot 3 \cdot 4} D_2 y^4,$$

and so on until we come back to the first Sturmian remainder of $(x, y)^i$, the irreducible part of which (or we may call it the Sturmian Auxiliary Proper) is the Hessian differentiated down from being of the degree $2i-4$ to the degree $i-2$, *i. e.* to half of what it was at first; and so in like manner every Sturmian Auxiliary Proper is, so to say, a Covariant differentiated down to half its original dimensions.

The above invariant and the following covariants may be called $V_0, V_1, V_2, \ldots$ respectively. The interesting point in question is that (to numerical factors *près*)

$$U_0 = V_0, \quad U_1 = \frac{d}{dx} V_1, \quad U_2 = \left(\frac{d}{dx}\right)^2 V_2, \quad U_3 = \left(\frac{d}{dx}\right)^3 V_3,$$

and so on.

So more generally for any two functions $f(x, y)$, $\phi(x, y)$, the irreducible part of the remainders obtained by common-measuring them with respect to x will all be derivatives in regard to x of covariants of the two given quantics. If we take for our quantics

$$(a, b, c, \ldots h, k, l \rangle\!\langle x, y)^i : (a', b', c', \ldots h', k', l' \rangle\!\langle x, y)^i,$$

the covariants in question will all be educts of (*i. e.* functions having for their leading coefficients) the successive resultants of the forms

$$[(a, \ldots h, k, l), (a' \ldots h', k', l')],$$

of the forms

$$[(a, \ldots h, k), (a', \ldots h', k')],$$

of the forms

$$[(a, \ldots h), (a', \ldots h')],$$

and so on, the discriminants of which may be called *partial resultants* of the given forms ; in a word, the simplified residues arising in the process of commonmeasuring in respect to one of their variables two given binary quantics are differential derivatives, in respect to that variable, of the educts of their partial resultants (of course with the understanding that the last simplified residue is the complete resultant itself).

This seems to point to the existence of some generalized statement of Sturm's theorem in which the same Educts as

above referred to shall appear, but where, instead of their derivatives in respect to one of the variables being made use of, perfectly general Emanants of them shall be employed as the Criterion functions. For I need hardly add that all Educts (although not written so as to show it in what precedes) are in fact symmetrical in respect to the two sides of the quantic to which they belong.

On various *à priori* grounds I suspect the generalized theorem to be as follows. If $X_{2\mu}$ is the covariant (of degree 2μ) whose μth derivative in respect to x is a Sturmian Auxiliary Proper to $F(x, y)$, we may substitute throughout for all the values of μ, instead of each such derivative, the more general one $\left(f\dfrac{d}{dx} - g\dfrac{d}{dy}\right)^{\mu} X_{2\mu}$, where f and q are any assumed positive constants, of course with the understanding that the second criterion also is to be $\left(f\dfrac{d}{dx} - g\dfrac{d}{dy}\right)f$ in lieu of $\dfrac{dF}{dx}$. And the method of Sturm will still be applicable for finding the positions of the real roots of $\dfrac{x}{y}$ in $f(x, y) = 0$ when we use these more general derivatives as the criteria instead of Sturm's own. When $g = 0$ the theorem is that of Sturm ; when $f = 0$ it is an immediate deduction from this theorem applied to finding the positions of the root values of $\dfrac{y}{x}$, when it is borne in mind that the motions of $\dfrac{x}{y}$ and of $\dfrac{y}{x}$, as regards ascent and descent (excluding the moment for which either of these ratios is indefinitely near to zero) are inverse to each other. It is this that accounts for the negative sign which precedes g.

It is difficult to conceive by what theorem other than the assumed one the chasm between those extreme cases can be bridged over; and all analogy and all belief in continuity vetoes the supposition that no such bridge exists. "Divide *et impera*" is as true in algebra as in statecraft ; but no less true and even more fertile is the maxim " auge *et impera*." The more to do or to prove, the easier the doing or the proof.

November 19, 1877.

XXVI. *Electromagnetic and Calorimetric Absolute Measure-*
ments: the Absolute Value of Siemens's Unit of Resistance in
Electromagnetic Measure; the Relation between the Current-
work and the Heat-evolution in stationary Galvanic Currents;
and the Absolute Values of some constant Hydroelectromotive
Forces in Electromagnetic Measure. (*Condensed Comparison*
of the Results of a Series of Investigations.) *By* H. F.
WEBER, *Professor of Mathematical and Technical Physics*
at the Federal Polytechnic Academy of Zurich.

[Concluded from p. 139.]

IV. *Absolute Values of constant Hydroelectromotive Forces.*
(*Third Procedure for the absolute Determination of the*
Siemens Unit of Resistance.)

HAVING given in the foregoing the experimental proof
that the mechanical work consumed in the flow of sta-
tionary galvanic currents, when there is no other action of the
current, indeed finds its exact equivalent in the heat produced
by the current, a *new* path can be entered in order to deter-
mine the absolute values of galvanic resistances and constant
hydroelectromotive forces.

I. Measure the quantity of heat Q, which is produced by
the current i (measured absolutely, according to any system
whatever) in a conductor whose resistance is w, which forms
part of a circuit, during the time z; the absolute value of the
resistance w (measured according to the same system) is then
to be calculated from the equation

$$\mathrm{J}\mathrm{Q} = i^2 w z.$$

II. If the ratio of the resistance w to the sum of the resist-
ances w_1 of the rest of the circuit be then ascertained by means
of an appropriate procedure, the heat produced in the entire
circuit by the constant current i during the time z will be ob-
tained in the expression

$$\Sigma(\mathrm{Q}) = \left(1 + \frac{w_1}{w}\right).$$

If E denotes the sum of all the electromotive forces of the cir-
cuit, according to Ohm's and Joule's laws, combined,

$$\mathrm{J}\Sigma(\mathrm{Q}) = i^2 \Sigma(w) z = i\mathrm{E}z$$

holds good. For the absolute determination of the sum of the
electromotive forces in the circuit, or, more briefly, for the

absolute determination of the electromotive force active in the circuit, we obtain the equation

$$\mathrm{JQ}\left(1+\frac{w_1}{w}\right)=i\mathrm{E}z.$$

III. If we then determine, according to one of the galvanic methods usually employed, the value of the same electromotive force in relative measure (let e denote this value)—say, on the basis of the absolute electromagnetic unit of current and the Siemens unit of resistance,—then by combining the two measurements a new mean will be obtained to determine the absolute electromagnetic value of the relative unit of resistance made use of: the absolute value of the latter is

$$1 \text{ S. M. U.} = \frac{\mathrm{E}}{e}.$$

provided that the current-intensity i in the equation

$$\mathrm{JQ}\left(1+\frac{w_1}{w}\right)=i\mathrm{E}z,$$

which served for determining E, was likewise measured according to electromagnetic measure.

I have carried out a series of absolute and relative measurements of the electromotive forces of the elements of Daniell and Bunsen according to procedures II. and III., in order to be able to derive the absolute value of the S. M. U. by a third method totally different from the two already described. When selecting this process I had a secondary object also in view— to submit to as rigorous a trial as possible the correctness of a singular result obtained by M. Favre, which directly contradicts a great number of galvanic experiences. In this operation one of my pupils, M. Rudio, rendered me important assistance.

In the circuit of the Daniell or Bunsen pile employed, consisting of 7–10 elements, the platinum wire wound upon a hardgum frame, previously used, was placed in the already-mentioned water calorimeter. The resistance w of the platinum wire was known accurately for all the temperatures employed. The resistance w_1 of the rest of the circuit, in which, as an essential part of the resistance, the pile was comprised, was determined simultaneously with the electromotive force of the latter, by a process resembling that described by Mr. Mance*. A path for the current was constructed after the fashion of Wheatstone's bridge-process; the place of the

* Proceedings of the Royal Society, vol. xix. 1871, p. 248.

pile in Wheatstone's scheme was taken by a sensitive galva-nometer; that of the resistance to be measured in Wheat-stone's plan was occupied by the pile whose resistance and electromotive force were to be measured, the single tangent-compass ($R = 165 \cdot 7$ millim.), and the other wire resistances which were included in the resistance w_1. The resistances of the measuring-wire and the galvanometer branch had been accurately ascertained. With the bridge open and the rest of the circuit closed the galvanometer and the tangent-compass indicated certain deflections. The point of connexion of the bridge-wire with the measuring-wire was now so chosen that the deflection of the sensitive galvanometer remained invari-able whether the bridge was open or for an instant closed. As soon as this point was found, according to known rules, first, the resistance w_1 (in S. M. U.), could be determined, which the pile employed, the tangent-compass, and the wires belonging to them possessed at that determinate current-intensity i_1 which had been indicated by the tangent-compass with the bridge open; secondly, the electromotive force exhibited by the pile when traversed by the current i_1 could be calculated in relative measure (founded on the absolute electromagnetic current-unit and Siemens's unit of resistance).

After this the *absolute* value of the same electromotive force was determined, by means of the amount of heat which it ge-nerated in its circuit by a current i, maintained constant (which was always approximately $= i_1$), during the time z. For this purpose the pile, the tangent-compass, and the wires which were also comprised in the resistance w_1 were combined with the platinum resistance w in the calorimeter to form a circuit, through which the constant current i then passed during the time z. The quantity of heat Q, which this current would have called forth during this time in the calorimeter if the platinum resistance had possessed, not the alternating tempe-ratures of the calorimeter, but the constant temperature t_a of the environment, is, according to equation (2) in section III.,

$$Q = M c_a [t - t_0 + B(\bar{t} - t_a)z] = \frac{i^2 w_a z}{J}.$$

This heat was calculated from M, c_a, t, $\bar{t}$, t_0, t_a, and z by the previously indicated process.

Immediately after the conclusion of the calorimetrical mea-surement, the resistance w_1 and the electromotive force e were measured a second time in relative measure according to the above-described procedure, in order to control any variation in the two quantities that might have taken place during the

time z and bring it into the calculation. Such variations were regularly ascertained; but they did not exceed very narrow limits. As these small variations of w_1 and e have their physical cause in processes which run proportionally with the time, it is permitted to put, instead of their mean values during the time z, the mean values given by the initial and final observations. If the initial values of the relative electromotive force and the resistance w_1 be called respectively e' and w_1', and the final values e'' and w_1'', understanding by E the mean value of the absolutely, and by e that of the relatively measured electromotive force, and let w_a represent the value of the platinum resistance corresponding to the temperature t_a of the environment of the calorimeter, we have for the determination of the quantities E and e the two equations :—

$$\text{JQ}\left[1 + \frac{w_1' + w_1''}{2w_a}\right] = i\text{E}z\,;$$

and

$$e = \frac{e' + e''}{2}.$$

From this we can derive the absolute value of Siemens's resistance-unit :

$$1\ \text{S. M. U.} = \frac{\text{E}}{e}.$$

I give in the following the results of the experiments which I have carried out with the cooperation of M. Rudio. In the calculation of the experiments J was supposed equal to 428·55 m.-k., equal to the mean of the values resulting from our experiments on galvanic heat-evolution and from the experiments on the thermal behaviour of the permanent gases.

Experiment 1. Bunsen's element—freshly amalgamated zinc, sulphuric acid of sp. gr. 1·035, commercial nitric acid of sp. gr. 1·365, gas-coal.

$$w_1' = 7{\cdot}683\ \text{S. M. U.},\quad w_1'' = 7{\cdot}449\ \text{S. M. U.},\quad e' = 19{\cdot}873,$$

$$e'' = 19{\cdot}734,\quad \text{E} = 18{\cdot}885 \times 10^{10}\left(\frac{\text{millim.}^{\frac{3}{2}}\,\text{milligr.}^{\frac{1}{2}}}{\text{sec.}^2}\right).$$

<div align="center">Mean values.</div>

$$\overline{w}_1 = 7{\cdot}566\quad e = 19{\cdot}804.$$

<div align="center">Result.</div>

$$1\ \text{S. M. U.} = \frac{\text{E}}{e} = 0{\cdot}9536 \times 10^{10}\left(\frac{\text{millim.}}{\text{sec.}}\right).$$

Experiment 2. The same element with the same filling.

$$w_1' = 7\cdot411 \text{ S. M. U.}, \quad w_1'' = 7\cdot279 \text{ S. M. U.}, \quad e' = 20\cdot094,$$

$$e'' = 20\cdot007, \quad \text{E} = 19\cdot150 \times 10^{10} \left(\frac{\text{millim.}^{\frac{3}{2}} \text{ mgr.}^{\frac{1}{2}}}{\text{sec.}^2} \right).$$

Mean values.
$$\overline{w}_1 = 7\cdot345 \quad e = 20\cdot050.$$

Result.
$$1 \text{ S. M. U.} = 0\cdot9552 \times 10^{10} \left(\frac{\text{millim.}}{\text{sec.}} \right).$$

Experiment 3. Daniell's element—freshly amalgamated zinc, sulphuric acid of sp. gr. 1·035, concentrated solution of sulphate of copper, copper.

$$w_1' = 6\cdot949 \text{ S. M. U.}, \quad w_1'' = 7\cdot081 \text{ S. M. U.}, \quad e' = 11\cdot952,$$

$$e'' = 11\cdot741, \quad \text{E} = 11\cdot286 \times 10^{10} \left(\frac{\text{millim.}^{\frac{3}{2}} \text{ mgr.}^{\frac{1}{2}}}{\text{sec.}^2} \right).$$

Mean values.
$$\overline{w}_1 = 7\cdot015 \quad e = 11\cdot847.$$

Result.
$$1 \text{ S. M. U.} = 0\cdot9526 \times 10^{10} \left(\frac{\text{millim.}}{\text{sec.}} \right).$$

Experiment 4. The same element with the same filling.

$$w_1' = 6\cdot831 \text{ S. M. U.}, \quad w_1'' = 7\cdot125 \text{ S. M. U.}, \quad e' = 11\cdot887,$$

$$e'' = 11\cdot739, \quad \text{E} = 11\cdot317 \times 10^{10} \left(\frac{\text{millim.}^{\frac{3}{2}} \text{ mgr.}^{\frac{1}{2}}}{\text{sec.}^2} \right).$$

Mean values.
$$\overline{w}_1 = 6\cdot978 \quad e = 11\cdot814.$$

Result.
$$1 \text{ S. M. U.} = 0\cdot9579 \times 10^{10} \left(\frac{\text{millim.}}{\text{sec.}} \right).$$

Experiment 5. Daniell's element—freshly amalgamated zinc, concentrated solution of sulphate of zinc, concentrated solution of sulphate of copper, copper.

$$w_1' = 16\cdot598 \text{ S. M. U.}, \quad w_1'' = 16\cdot039 \text{ S. M. U.}, \quad e' = 11\cdot453,$$

$$e'' = 11\cdot450, \quad \text{E} = 10\cdot954 \times 10^{10} \left(\frac{\text{millim.}^{\frac{3}{2}} \text{ mgr.}^{\frac{1}{2}}}{\text{sec.}^2} \right).$$

Mean values.
$$\overline{w}_1 = 16\cdot319 \quad e = 11\cdot451.$$

Result.
$$1 \text{ S. M. U.} = 0\cdot9565 \times 10^{10} \left(\frac{\text{millim.}}{\text{sec.}} \right).$$

The determinations executed according to this third method, of the absolute electromagnetic value of Siemens's mercury unit, have given the following results:—

$$1 \text{ S. M. U.} = 0 \cdot 9536 \times 10^{10}$$
$$\text{''} \qquad = 0 \cdot 9552 \text{ ''}$$
$$\text{''} \qquad = 0 \cdot 9526 \text{ ''}$$
$$\text{''} \qquad = 0 \cdot 9579 \text{ ''}$$
$$\text{''} \qquad = 0 \cdot 9565 \text{ ''}$$

The mean value from these experiments amounts to

$$\mathbf{1 \text{ S. M. U.}} = \mathbf{0 \cdot 9550} \times 10^{10} \left(\frac{\text{millim.}}{\text{sec.}} \right).$$

For facility of review, I place the final results for the absolute value of the S. M. U. together. We have found the absolute electromagnetic value of:—

$$1 \text{ S. M. U.} = 0 \cdot 9545 \times 10^{10} \left(\frac{\text{millim.}}{\text{sec.}} \right)$$

from 18 series of experiments, in which the variable currents generated by magneto-induction were employed;

$$1 \text{ S. M. U.} = 0 \cdot 9554 \times 10^{10} \left(\frac{\text{millim.}}{\text{sec.}} \right)$$

from 24 series of experiments, in which the variable currents called forth by sudden voltaic induction were employed; and

$$1 \text{ S. M. U.} = 0 \cdot 9550 \times 10^{10} \left(\frac{\text{millim.}}{\text{sec.}} \right)$$

from 5 series of experiments, in which the heat-production of stationary galvanic currents was used.

The general mean,

$$\mathbf{1 \text{ S. M. U.}} = \mathbf{0 \cdot 9550} \times 10^{10} \left(\frac{\text{millim.}}{\text{sec.}} \right),$$

is only $\frac{1}{7}$ per cent. greater than the result found by Messrs. Maxwell, Jenkin, and Stewart. After these results I hold that the questions of the true absolute value of the S. M. U., and whether the British resistance-unit does or does not represent the value asserted, are settled. The true value of S. M. U. lies between $0 \cdot 9536 \times 10^{10} \left(\frac{\text{millim.}}{\text{sec.}} \right)$ and $0 \cdot 9550 \times 10^{10} \left(\frac{\text{millim.}}{\text{sec.}} \right)$; and the British unit represents, neglecting *very minute* differences possibly still present, the value asserted, $10^{10} \left(\frac{\text{millim.}}{\text{sec.}} \right)$.

When an observer finds the same result in three different

ways, and employing three quite different natural laws—when, further, this result but very slightly differs from that of another group of observers who worked according to a fourth, essentially different method, certainly it can be pretty safely maintained that the result so found is correct.

In instituting this last series of experiments, besides ascertaining the absolute value of the S. M. U., I pursued also, as I have already intimated, another aim, which, in conclusion, I will briefly explain.

M. Favre has repeatedly determined with the aid of the mercury calorimeter the quantities of heat developed by the most various electromotive forces in their circuits during the time in which they consume equal quantities of zinc—namely, the quantity which is chemically equivalent to the unit of mass of hydrogen. As the result of his experiments, he found that *the ratio of those quantities of heat gives quite another value than does the ratio of the corresponding electromotive forces when measured galvanometrically.* Thus, according to M. Favre, the quantities of heat which the elements of Daniell and Grove produce in their circuits during the time within which they consume 1 equivalent of zinc are 23993 and 46447 units. The ratio of these numbers is 1 : 1·93, while the electromotive forces of the Daniell and Grove elements stand (according to all galvanometric measurements hitherto executed) in the ratio of from 1 : 1·68 to 1 : 1·70. This result of M. Favre's directly contradicts certain galvanic laws which are universally regarded as resting on a secure foundation, as will be evident from the following consideration:—

If E denotes the hydroelectromotive force of a circuit, w the sum of all the resistances of the circuit, and Q the sum of all the quantities of heat which the constant current i calls forth in the circuit during the time z, then, according to Joule's law (which we have demonstrated under section III. to be correct),

$$JQ = i^2 wz;$$

or, if, according to Ohm's law, we put $iw = E$,

$$JQ = iEz.$$

If α denotes the electrochemical equivalent of zinc, the quantity m of zinc which is consumed within the element during the time z becomes, according to Faraday's law of electrolysis,

$$m = \alpha i z.$$

Therefore the total heat Q produced in the entire circuit by the

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electromotive force E during the time that within the element
the quantity m of zinc is consuming is

$$Q = \frac{Em}{Ja}.$$

Hence, if the galvanic laws of Joule, Ohm, and Faraday are
universally true, the quantities of heat Q_1 and Q_2 which two
different electromotive forces E_1 and E_2 develop in their cir-
cuits during the time they consume *equal* quantities of zinc
*must be in exactly the same proportion as the electromotive
forces E_1 and E_2. Consequently M. Favre's measurements and
the three laws mentioned are irreconcilable with each other.*

M. Favre's results are *refuted* by the above-stated determi-
nations. The relative values of the electromotive forces, mea-
sured by a galvanometric method, have been found to be:—
For Bunsen's element, in the mean,

$$e_1 = 19 \cdot 927.$$

For Daniell's element with sulphuric acid, in the mean,

$$e_2 = 11 \cdot 830.$$

For Daniell's element with sulphate of zinc,

$$e_3 = 11 \cdot 451.$$

And the absolute values of these electromotive forces, deter-
mined simultaneously by the heat generated in the entire
circuit, have given:—
For Bunsen's element, in the mean,

$$19 \cdot 017 \times 10^{10} \left(\frac{\text{millim.}^{\frac{3}{2}} \text{ milligr.}^{\frac{1}{2}}}{\text{sec.}^2} \right) = E_1.$$

For Daniell's element with sulphuric acid,

$$11 \cdot 301 \times 10^{10} \left(\frac{\text{millim.}^{\frac{3}{2}} \text{ milligr.}^{\frac{1}{2}}}{\text{sec.}^2} \right) = E_2.$$

For Daniell's element with sulphate of zinc,

$$10 \cdot 954 \times 10^{10} \left(\frac{\text{millim.}^{\frac{3}{2}} \text{ milligr.}^{\frac{1}{2}}}{\text{sec.}^2} \right) = E_3.$$

From these we get for the ratio of the galvanometrically
measured electromotive forces and the electromotive forces
measured by their heat-evolution the values

$$\frac{e_1}{e_2} = 1 \cdot 684, \quad \frac{e_1}{e_3} = 1 \cdot 740, \quad \frac{e_2}{e_3} = 1 \cdot 033,$$

$$\frac{E_1}{E} = 1 \cdot 683, \quad \frac{E_1}{E_3} = 1 \cdot 737, \quad \frac{E_2}{E_3} = 1 \cdot 031,$$

numbers which rigorously correspond to the deductions from the laws of Ohm, Joule, and Faraday. The cause of Favre's result being so seriously faulty lies probably, in great part, in the circumstance that, in all his calorimetric investigations, he made use of the *mercury calorimeter*, with the use of which a whole series of uncertainties are necessarily connected, and which it should be a maxim not to employ. For all galvano-calorimetric investigations in which the duration of the heat-evolution can be chosen entirely at discretion, and so the heat produced can be made as great as we please, the simple water calorimeter, managed with nicety, is by far the most reliable, and, for many reasons, even preferable to Bunsen's ice calorimeter. The numerous measurements instituted by M. Favre many years since, respecting heat-production by galvanic currents and electromotive forces, were very probably all vitiated by an error of the same order as were the values given by him for the heat developed by Daniell's and Grove's elements. Should a secure basis be obtained in this department, nothing remains but to repeat with more accurate methods all the more important of his measurements.

The unit of length employed in these investigations is the millimetre of the cathetometer of the Physical Laboratory at Zurich ; the time-unit is the second of mean time ; the Siemens resistance-unit is the No. 1914, which I obtained from M. W. Siemens at the commencement of the investigation, and which was most carefully compared with all the other resistances employed.

Zurich, August 1877.

XXVII. *Rain-Clouds and Atmospheric Electricity. By* W. E. AYRTON *and* JOHN PERRY, *Professors in the Imperial College of Engineering, Tokio, Japan.*

To the Editors of the Philosophical Magazine and Journal.

<div style="text-align:right">The Imperial College of Engineering,
Tokio, Japan, December 8, 1877.</div>

GENTLEMEN,

GIVING all due weight to the theories of thermoelectric currents produced by rotation of the earth under the sun, and of currents which might be produced by moving electrified shells of air, we have always thought that these sources of electric disturbance on the earth were far too inconsiderable to give rise to the phenomena of earth-currents and of atmospheric electricity, and also totally inadequate to account for currents of sufficient intensity to produce terrestrial magnetism. We think that there cannot be any

explanation of these phenomena which does not take into account the fact that the earth and other members of the solar system are the electrified coatings of condensers, since, although the mutual coefficients of induction between the different members of the solar system may be very small compared with the capacities of long telegraph-cables, still the differences of potential between the sun and the planets may be extremely great, so that the charges in the condensers in question may be so large that a slight change in the capacities, produced by rotations or changes in the positions of the planets, may set up in the bodies themselves electric currents of considerable magnitude.

We are at present engaged in the solution of the problem to determine mathematically what is the strength of the currents produced in the earth as it rotates under the inductive action of the sun; and we may mention that the moderate conductivity of the earth, combined with the probability of the existence of an iron core, will enable us very shortly to present the results drawn from our theory in a numerical form. In the meantime, however, we desire to show how it follows from this theory that movements in the atmospheres of the earth and sun, and especially the motions of rain-clouds, or clouds of vapour, are connected with the phenomena of atmospheric electricity and earth-currents. The connexion of these latter phenomena with earthquakes, which we dwelt on in our recent paper on observations of atmospheric electricity, will more suitably be taken up again in our next paper.

When a dielectric is composed of air at different pressures, or of a mixture of gases, our experiments (recently communicated to the Asiatic Society of Japan in a paper on the Specific Inductive Capacity of Gases) showed that K, the specific inductive capacity of the medium, is different at different parts, so that (see C. Maxwell's 'Electricity') the fundamental equation connecting the potential V and ρ the charge of electricity per unit volume of the medium at different points, is

$$\frac{d}{dx}\left(K \cdot \frac{dV}{dx}\right) + \frac{d}{dy}\left(K \cdot \frac{dV}{dy}\right) + \frac{d}{dz}\left(K \cdot \frac{dV}{dz}\right) + 4\pi\rho = 0. \quad (1)$$

Consequently, if there is no real charge in the medium, we have

$$\frac{d}{dx}\left(K \cdot \frac{dV}{dx}\right) + \frac{d}{dy}\left(K \cdot \frac{dV}{dy}\right) + \frac{d}{dz}\left(K \cdot \frac{dV}{dz}\right) = 0. \quad . \quad . \quad (2)$$

Now, if we imagine the non-homogeneous dielectric to be all replaced by dry air at 760 millimetres pressure and at 0° C. temperature, and if at any point where the potential is V there

is an imaginary charge ρ' such as would produce the actual distribution of potential that we have in the real case, then

$$\frac{d^2V}{dx^2} + \frac{d^2V}{dy^2} + \frac{d^2V}{dz^2} + 4\pi\rho' = 0. \quad . \quad . \quad (3)$$

From equations (2) and (3) we find

$$4\pi K\rho' = \frac{dK}{dx} \cdot \frac{dV}{dx} + \frac{dK}{dy} \cdot \frac{dV}{dy} + \frac{dK}{dz} \cdot \frac{dV}{dz};$$

or if ρ is the density per unit volume of a real charge at any point of the non-homogeneous dielectric, then

$$4\pi K\rho' = 4\pi\rho + \frac{dK}{dx} \cdot \frac{dV}{dx} + \frac{dK}{dy} \cdot \frac{dV}{dy} + \frac{dK}{dz} \cdot \frac{dV}{dz}.$$

Also if at any place there is a distinct separation by a surface of one dielectric from another, ordinary air from very moist air for example, then the resultant force on one side of the surface must be greater than that on the other. Thus, if the resolved part of the resultant force in a direction at right angles to this surface be F in the first medium and F′ in the second, and if K and K′ are the specific inductive capacities,

$$KF = K'F'.$$

In fact it is the same as if both media were dry air as above, and an apparent charge, of density σ', were given to the surface, such that

$$4\pi\sigma' = \left(1 + \frac{K'}{K}\right) F'$$

$$= \left(\frac{K}{K'} - 1\right) F.$$

If at the surface there is a real charge of density σ, then

$$KF = K'F' + 4\pi\sigma;$$

and the action is as if both media were air as above, and an apparent charge of density σ' were given to the surface, where

$$4\pi\sigma' = \frac{4\pi\sigma}{K} + \left(\frac{K'}{K} - 1\right) F'$$

$$= \frac{4\pi\sigma}{K} + \left(1 - \frac{K}{K'}\right) F.$$

These formulæ may be used, when we know the state of the atmosphere at every point at every time (that is, when the specific inductive capacity K is known), in order to find from any given initial distribution of potential the changes which occur when the state of the atmosphere is changed.

It is known that, from observations of atmospheric electricity and from observations of earth-currents, atmospheric

changes may be predicted (see our paper on Observations of Atmospheric Electricity, read before the Asiatic Society of Japan, April 25, 1877). And we see from the above equations why this should be the case, since changes in the state of the atmosphere, whether brought about by actual motions, or by alterations of density from cooling, or from other causes, must produce changes in the specific inductive capacity of the dielectric, and consequently alterations of the potential of the earth in the neighbourhood. Assuming dry air at 760 millims. pressure and at 0° C. temperature to have a specific inductive capacity unity, then as we mix some aqueous vapour with it the specific inductive capacity increases and becomes larger than 1; and in addition, as some of the vapour condenses, we know that the globules of water, excessively small at the beginning, soon increase in size, so that, as the specific inductive capacity of water is some millions of times that of air, the mean specific inductive capacity of the space is immensely increased; and hence we see that the cooling of the atmosphere and the formation of clouds, or the approach of clouds, may occasion great changes in the distribution of atmospheric potential at any place, and consequently give rise to strong earth-currents. And if the cloud has no charge of its own, the direction of these currents will be such that positive electricity will flow from the place from which the cloud is passing to that from which it came; for since the earth is known to be negatively electrified with regard to space, the introduction of a cloud or other dielectric of greater specific inductive capacity than air must make the potential of the part of the earth's surface underneath it less negative than before—that is, must raise the potential.

Although mere changes in the density of the air will, for the reasons given above, be sufficient to produce earth-currents, still we should imagine that the changes in the atmospheric potential commonly observed are due, not so much to the direct change of the specific inductive capacity with change of density, as to the much greater changes that must be produced in the capacity by the formation of clouds resulting from the change of temperature and density; so that we should expect that observations of atmospheric electricity will be of greater use in the predicting of rain and snow than of wind-storms.

As the atmosphere does not altogether consist of non-conducting matter, portions of it, especially cloudy portions, are capable of acquiring electric charges, through changes of temperature or motions of the atmosphere; and these portions subsequently become more or less conducting through changes of

temperature : we here see an explanation of how thunder-clouds
may be formed. It is probable, however, that in the ordinary
phenomena of atmospheric electricity and earth-currents the
real charges possessed by portions of the atmosphere may have
but little effect ; but on this point we cannot at present express
a decided opinion.

<div style="text-align:center">

We beg to remain, Gentlemen,

Very truly yours,

JOHN PERRY,

W. E. AYRTON.

</div>

XXVIII. *On a new Modification of the Bichromate Battery.*
By H. C. RUSSELL*.

[Plate VIII. fig. 14.]

Sydney Observatory,
May 31, 1877.

MY DEAR SIR,

YOU were kind enough to advise me, when I was in
England, about the purchase of a large Ruhmkorff coil.
In using it, I have had the ordinary bichromate-of-potash cells
to develop the electricity, and of course have had to suffer the
inconveniences which attend the use of a cell which falls off so
quickly. This has induced me to look for a more constant form
of electric generator ; and I have found one that is *perfectly
constant* in its action, and will remain so as long as the solution
and zinc are supplied. I enclose a section-drawing (Pl. VIII.
fig. 14); and you will see I have adopted a modification of one
of Daniell's earliest ideas for obtaining a constant current. The
solution used is the ordinary bichromate of potash, in the
arrangement shown, which perhaps needs little explanation ;
but I may say that a *drop of solution per second* keeps the cell
in full and steady work. The drawing is at fault in one
respect ; the only *space* in the cell for solution is between the
plates, not, as shown, all round the plates : the object of this is
to make all the solution pass the face of the zinc. You will
observe that, supposing the waste-tap shut and the drop-
tap above opened, the solution accumulates in the cell till
it comes to the level of the overflow-pipe ; and then, for every
drop which goes in, one goes out ; but as the overflow-pipe
begins at the bottom of the solution, it is the used or waste
solution which must go out. When the battery is not required,
the waste-tap is opened and the other shut, and the battery-
cell is left perfectly inactive and ready for the next time it
is wanted. There is of course some additional first cost ; but

* Communicated by Dr. Huggins, F.R.S.

I find the convenience and certainty of action far overbalance this.

I send it in the hope that you may find it useful; but should you know of any more convenient electric generator, I shall be much obliged if you will let me know.   On the other hand, if the foregoing is something new and you think it worth publication, I leave it entirely in your hands to do as you like.   I had hoped to be able to send you a copy of some work I have been doing with the spectroscope; but the mail closes before I am ready, and I await another opportunity.

<div style="text-align:center">Believe me, my dear Sir,</div>

<div style="text-align:center">Yours faithfully,</div>

Dr. Huggins,   H. C. RUSSELL.
    &c.   &c.

XXIX. *Contributions to the Study of States of Aggregation.* *By* A. RITTER [*].

[Plate VIII. figs. 1–13.]

§ 1. *Temperature-surface of Air.*

IF a kilogram of air at rest is confined in a cylinder by a movable piston, its pressure p, volume v, and absolute temperature T are, by the gaseous laws, connected by the the equation

$$pv = RT, \quad \ldots \ldots \ldots \quad (1)$$

the constant R being equal to 29·27 when v is expressed in cubic metres and p in kilograms weight per square metre. This equation shows that the quotient $\frac{pv}{T}$ has always a constant value, however the state of the air may be altered by shifting the piston or applying heat.

For the absolute temperature of the air we have from the above equation the expression

$$T = \frac{pv}{R}. \quad \ldots \ldots \ldots \quad (2)$$

The magnitude T appears in this equation as a function of the two variables p, v; and the law of the alteration of T with p and v can be exhibited geometrically by a curved surface. If the point in the horizontal plane O X Y whose coordinates are p, v is found, and a perpendicular to the plane of length T drawn through it, the extremity J of this perpendicular

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[*] From Poggendorff's *Annalen*, No. 10, 1877; translated and communicated by Robert E. Baynes, M.A., Senior Student of Christ Church, Oxford, and Lee's Reader in Physics.

may be taken to represent the state of the air at the given moment (fig. 1, Pl. VIII.). Suppose this construction repeated for all values of p, v; then the geometrical locus of all the points on which the extremity J can lie is a curved surface, which for shortness may be called the "*temperature-surface.*" To every point of this surface corresponds a particular state of the air, since a particular value of each of the variables p, v, T is given when the position of the point is given.

Through the point J draw a plane parallel to the vertical plane O Y Z; the temperature-surface is cut by this plane in a *straight* line whose inclination ϵ to the plane of $p\,v$ is given by the equation

$$\tan \epsilon = \frac{d\mathrm{T}}{dp} = \frac{v}{\mathrm{R}}, \quad . \quad . \quad . \quad . \quad . \quad (3)$$

in which v is to be considered constant.

Again, draw a plane through J parallel to the vertical plane O X Z; its line of intersection with the surface is also *straight*, its inclination being given by

$$\tan \omega = \frac{d\mathrm{T}}{dv} = \frac{p}{\mathrm{R}}, \quad . \quad . \quad . \quad . \quad (4)$$

wherein p must be taken constant.

Thus the lines of constant volume and the lines of constant pressure on this *curved* surface form two systems of *straight* lines. The temperature-surface can therefore be conceived as the geometrical locus of all the intersections of these two systems of lines.

If, finally, we draw a *horizontal* plane through the point J, the temperature-surface will be cut by it along a curved line which represents a line of constant temperature (as it corresponds to the equation $\mathrm{T} = \mathrm{const.}$), and may therefore be termed an "*isothermal*" (fig. 2). The equation of these isothermals may also be given in the form

$$pv = \mathrm{const.}, \quad . \quad . \quad . \quad . \quad . \quad (5)$$

whence it appears that an isothermal lies in a horizontal plane and is a rectangular hyperbola.

If we consider the temperature-surface as the face of a mountain, the isothermals will be represented by curved *horizontal* paths along its slope, while the lines of constant volume and the lines of constant pressure are *straight* paths leading directly up the slope. Each given alteration in the state of the air may then be looked upon as a movement over the mountain by a given path whose successive points represent the successive states through which the air passes.

§ 2. *Isothermals of Water-vapour.*

The temperature-surface of the so-called perfect gases is, as was shown in the preceding paragraph with reference to atmospheric air, a surface *continuously* curved in *all* its parts. The temperature-surface of steam is, on the contrary, a curved surface with *edges*.

As its temperature falls, steam passes into the liquid and solid states of aggregation. To these changes correspond changes in the law of curvature of the temperature-surface, which will accordingly appear as a curved surface made up of several surfaces of continuous curvature.

We get a clear conception of the difference between vapours and perfect gases by likening their temperature-surfaces to mountain-faces as before. In the higher regions the forms of both mountains would most probably be approximately the same, since we may assume that at very high temperatures steam behaves like a perfect gas. Considerable differences between the two forms, however, will make an appearance lower down, since, in the mountain which represents the behaviour of water in its three states of aggregation, the uniformity of the continuously curved slope is broken by sharp-edged cliffs and steep walls of rock that stand out and project cornice-like, wholly changing the character of the landscape in the lower regions. Consequently also the horizontal paths, that run along the mountain-slope and represent the isothermals, will in the lower regions differ very considerably in form from the isothermals of perfect gases.

If *superheated* steam undergoes *isothermal* compression, and the law of alteration of its pressure p with the volume v during the motion of the piston is represented geometrically by a line, then this line runs on at first just as in the case of atmospheric air. At the point M, however, that corresponds to the passage of the vapour into the *saturated* state, the line will form an angle (fig. 3). Condensation begins at this position of the piston, and the pressure p remains *constant* when the piston is pushed further in. The following part of the isothermal will therefore be a *straight* line *parallel* to the volume-axis O V. This straight line M N extends to the point N corresponding to the condensation of the last particle of steam. Here the isothermal forms another angle ; for the pressure of *water* increases with extraordinary rapidity when its volume is diminished. This last piece of the isothermal will therefore be a curve that rises up very steeply from the axis of abscissæ.

In passing from the isothermal T to the isothermal $T + dT$

each of the two angular points M and N will describe a line-element that belongs to an *edge* of the temperature-surface (fig. 4). Analogous edge-formations will appear on the surface at those points that correspond to the passage from the liquid into the solid state of aggregation. Hence it follows that the temperature-surface for water in its three states of aggregation *cannot* be represented, as that of perfect gases, by *a single* equation of simple form. Such an equation will rather represent in all cases only a portion, more or less limited, of the whole temperature-surface.

§ 3. *Isobars and Isothermals of the Ice-region.*

If a vertical plane is drawn parallel to the vertical tempe-rature-axis O T and to the horizontal volume-axis O V, and therefore perpendicular to the horizontal pressure-axis O P, its intersection with the temperature-surface is an " *isobar* " or line of constant pressure. Such an isobar will in general contain two straight horizontal lengths or segments, of which the upper corresponds to the *vaporization* and the lower to the *freezing* of water. As horizontal lines on the temperature-surface represent isothermals, each of these horizontal seg-ments is also an *isothermal* segment.

Thus, for instance, for the isobar corresponding to the con-stant pressure of one atmosphere the upper horizontal segment coincides with the isothermal for 100° C., and the lower with the isothermal for 0° C. (fig. 5). The length of the upper horizontal segment represents the expansion (about 1650-fold) that attends vaporization ; the length of the lower represents the expansion (about 9 per cent.) that occurs when water freezes. If a vertical line is drawn through a point of the lower horizontal segment, we see that under certain circum-stances three different temperatures can correspond to the same volume under given pressure, since the line of constant pressure is cut three times by the vertical line.

When the pressure increases, saturated steam experiences a *rise* in temperature, but melting ice a *fall* in temperature. Hence in the isobar for a pressure *greater* than one atmo-sphere the *upper* horizontal segment will take a *higher* position, but the *under* one a *lower* position. If, then, the temperature-surface is again represented by a mountain-face, that part of the mountain which represents the passage from the liquid to the solid state of aggregation will appear as a wall of rock that juts out and projects like a cornice (fig. 6).

Consider a horizontal plane drawn through the lower part of this temperature-mountain ; its line of intersection with the sur-face will be an *isothermal* of some such form as that in fig. 7;

just like the isobar, it has *four* angular points and *two* straight segments, the meaning of which is obvious when we consider the gradual transformation under an isothermal compression of a mass of water out of the state of superheated vapour, first into the solid and then into the liquid state of aggregation. The angular point M corresponds to the passage of the vapour from the superheated to the saturated state; the straight segment M N exhibits the gradual passage from the vapour to the solid state of aggregation (snow-formation). The point L marks the beginning of melting that only starts under higher pressure; and the straight segment L K represents the gradual passage from the solid into the liquid state of aggregation.

The isothermals of the *ice-region* are therefore distinguished from the isothermals for *higher* temperatures (that are represented in fig. 7 by the dotted line) by having *three* angular points N, L, K, instead of one, J, as in the latter. We pass from the one group to the other at the isothermal which corresponds to a temperature *higher* than 0° C. by 0·00744°; and this isothermal must therefore be counted among those of the ice-region. This limiting isothermal corresponds to the temperature (0°·00744 C.) at which water freezes or ice melts under a pressure equal to its vapour-tension (comp. § 5)[*].

Though isothermals may in general be likened to horizontal *paths* on a mountain-face, this comparison is unsuitable in the case of the isothermals of the ice-region as they stretch along the *under* surface of an overhanging cliff.

To the temperature 0°·00744 C. corresponds a vapour-tension of 0·006 atmosphere. If we draw the successive isobars (as in fig. 6) for continually *smaller* pressures, we shall find, as shown later, that for the pressure of 0·006 atmosphere the two straight segments coincide, since each will coincide with the straight segment of the isothermal for 0°·00744 C. For still smaller pressures the isobar takes the form in fig. 8. There is but *one* straight horizontal segment in this line; and it corresponds to the *direct* passage from the vapour into the solid state of aggregation.

§ 4. *Edges of the Temperature-surface.*

Consider the successive isobars drawn as in fig. 6, or the successive isothermals as in fig. 7; the *angular points* of these lines make up *edges* of the temperature-surface. These edges bound the three regions on the surface which correspond to the three different states of aggregation. These three regions, however, do not directly border on each other, but are sepa-

* [This peculiarity was first pointed out by Professor Plücker in the Proceedings of the Royal Society for 1874, p. 457.—Tr.]

rated by three regions of transition (figs. 9, 10, 11). Each of
these three regions of transition has the form of a cylindrical
surface whose generating lines are parallel to the volume-axis ;
viewed, therefore, in this direction, it appears a *line* (fig. 10).

As regards the water-edge J W and the steam-edge L D,
we may assume that they meet at a certain height (near the
isothermal for 410° C., according to Cagniard de la Tour), or
subside into the surface ; for it is exceedingly likely that at
very high temperatures there is no difference between the
liquid and gaseous states of aggregation *.

To each point of the temperature-surface correspond defi-
nite values of the coordinates p, v, T ; and by these three
values the state of the whole mass is in general unambigu-
ously defined. An important exception, however, occurs in
the case of those points that lie on the straight segment J K L
of the isothermal for 0°·00744 C. or of the isobar for 0·006
atmosphere. This segment (which is represented in fig. 10 by
the point J) corresponds to those values of the pressure and
temperature at which water can *simultaneously* exist in all
three states of aggregation †. If, then, the pressure, tempe-
rature, and volume of the whole mass be given by any point
on this segment, the internal condition of the mass is not suffi-
ciently defined, since a knowledge of the *total* volume is not
sufficient to determine the *proportions* in which steam, water,
and ice are mixed together. As, further, the intrinsic energy
(*die innere Wärme*) of steam is considerably greater than
that of water, and this latter than that of ice, the intrinsic
energy of the mixture is by no means defined by the position
of the point, an infinite number of values of the intrinsic
energy corresponding indeed to each single point of this
segment.

Thus, for example, the point K of this segment may repre-
sent the state of 1 kilogram of water which has increased 9 per
cent. in volume by *freezing throughout* ; or it may represent
the state of this mass after a 9-per-cent. increase in volume by
partial vaporization. In the latter state, however, the mass
would possess about 80 calories *more* of intrinsic energy than
in the former state.

This straight isothermal and isobaric segment J K L (figs. 9
and 11) forms an *edge* of the temperature-surface along its
whole length ; and as this edge is distinguished from all the
other lines and edges of the temperature-surface by the above-
mentioned remarkable properties, we shall call it in future the
"*principal edge of the temperature-surface.*" (Following J.

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* Andrews, Pogg. *Ann.* Ergänzbd. v. p. 64.
† The point J was on this account named the *triple point* by J. Thomson.

Thomson's nomenclature for the point J of fig. 10, we might also call it the *triple edge*).

We may further call the steam-edge L D the *cloud-edge*, since the beginning of condensation is marked by the formation of a cloud; and the water-edge J W may be called the *rain-edge* or the *dew-edge*, since the product of complete condensation exhibits itself as rain or dew.   The edge F J may be conceived as the line on which water begins to freeze, and may therefore appropriately be called the *frost-edge*.   The edge S K can be conceived as the line in which ice begins to melt, and may therefore be called the *melting-edge*.   The edge R K may be called the *rime-* or *snow-edge*, as the product of the direct transformation of aqueous vapour into the solid state appears as rime or snow.

To conclude, exact proportions could not be given in the above figures, from the nature of the case; for if, for example, the segment J K, so as to be perfectly discernible, were drawn even only *one millimetre* long, the segment K L would have a length of more than *two kilometres* on a diagram drawn exactly to scale.

§ 5. *Angles formed at the Principal Edge.*

By Clapeyron and Clausius's law the relation between the pressure and temperature of saturated steam can be expressed by the differential equation

$$\frac{dp}{dT} = \frac{r}{AuT}; \quad \cdot \quad \cdot \quad \cdot \quad \cdot \quad \cdot \quad \cdot \quad (1)$$

where r denotes the latent heat of steam, u the increment of volume that occurs on vaporization, $A = \frac{1}{424}$ the heat-equivalent of a kilogrammetre.   To the value $T = 273$ (or $t = 0$) correspond the values $r = 606\cdot5$ and $u = 210\cdot66$ [*].   In the isothermal for $0°$ C., therefore, the above differential coefficient takes the value

$$\frac{dp}{dT} = \frac{424 \times 606\cdot5}{210\cdot66 \times 273} = 4\cdot4715. \quad \cdot \quad \cdot \quad \cdot \quad \cdot \quad (2)$$

If the principal edge (represented in fig. 10 by the point J) lay *exactly* in the isothermal for $0°$ C., then would the above value be the tangent of the angle marked ϕ in that figure. As the point J really lies on the isothermal for $0°\cdot00744$ C. (as will appear later), the above coefficient, in order to represent tan ϕ *exactly*, requires a slight correction, which we can easily make by determining from tables by interpolation the values of r and u that correspond to $T = 273\cdot00744$ (or

[*] Zeuner's *Grundzüge der mechanischen Wärmetheorie.*

$t = 0.00744$) and repeating the above calculation with these values; we then find for the angle ϕ the more exact equation

$$\tan \phi = 4.483, \text{ or } \phi = 77° \, 25'. \quad . \quad . \quad . \quad (3)$$

Equation (1) can also be used for the passage from the solid into the liquid state of aggregation, if for u the increment (negative) of volume that occurs on melting and for r the latent heat of water are substituted. The latent heat of water has the value [*]

$$l = 80 \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (4)$$

for the pressure of one atmosphere and the melting-point (0° C.) corresponding to this pressure. In melting, the mass diminishes in volume by

$$\mathfrak{u} = 0.00109 - 0.001 = 0.00009 \text{ cubic metre.} \quad . \quad (5)$$

Hence, on substituting $-\mathfrak{u}$ for $+u$ and l for r in equation (1), we obtain the differential equation

$$\frac{dp}{dT} = -\frac{l}{A\mathfrak{u}T} \quad . \quad . \quad . \quad . \quad . \quad (6)$$

as the relation between the pressure and melting-point, l and $\mathfrak{u}$ being functions of T. To the temperature $T = 273$ correspond the values given in (4) and (5); and on substituting these we obtain for the value of the differential coefficient

$$\frac{dp}{dT} = -\frac{424 \times 80}{0.00009 \times 273} = -1380545. \quad . \quad . \quad (7)$$

To a pressure-increment therefore of 1380545 kilograms weight per square metre (or 133.6 atmospheres) corresponds a lowering of the melting-point by one degree C., if the differential coefficient does not alter in value for this change of temperature [†]. It follows that near the principal edge the band of the surface which lies between the frost- and melting-edges, and represents the mixture of ice and water (fig. 9), makes a very small angle with the horizontal plane, since an extremely small lowering of the isothermal for the melting-point corresponds to a very considerable increment of pressure. The angle marked ψ in fig. 10 differs therefore only very little from a right angle.

To a *diminution* of the pressure by the weight of 1 kilogram per square metre would, by equation (7), correspond a *rise* of the melting-point by $\frac{1}{1380545}$ degree. When, therefore, the pressure diminishes from 10333 to 62.58 kilograms weight

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[*] Wüllner, *Experimentalphysik*, 2nd ed. iii. p. 548.

[†] Clausius, *Mechanische Wärmetheorie*, 2nd ed. i. p. 173. [This result was first shown by Professor J. Thomson.—Tr.]

per square metre, the melting-point rises from the isothermal for $t = 0$ to that for

$$t = \frac{10333 - 62.58}{1380545} = 0.00744. \quad . \quad . \quad . \quad . \quad (8)$$

Since at this temperature the pressure of saturated vapour is also 62·58 kilograms weight per square metre (or 0·006 atmosphere), it follows that the principal edge coincides with the straight segment of the isothermal for $0°\cdot00744$ C. and of the isobar for 0·006 atmosphere pressure.

The *rime-edge* R K (fig. 9) can be conceived as the line along which the direct passage of ice into the gaseous state begins. In applying equation (1) to the sublimation of ice, we have to put $r + l$ in place of r, and $u - \mathfrak{u}$ in place of u; then for the relation between the pressure and the sublimation-point of ice we have the differential equation

$$\frac{dp}{dT} = \frac{r + l}{A(u - \mathfrak{u})T} \quad . \quad . \quad . \quad . \quad . \quad (9)$$

In the isothermal for $0°$ C. this differential coefficient takes the value

$$\frac{dp}{dT} = \frac{424 \times (606.5 + 80)}{(210.66 - 0.00009) \times 273} = 5.06, \quad . \quad . \quad (10)$$

from which the corresponding value for the isothermal for $t = 0\cdot00744$ differs by an insignificant quantity. For the angle marked ω in fig. 10 we therefore have

$$\tan \omega = 5\cdot06, \text{ or } \omega = 78° \ 50'. \quad . \quad . \quad . \quad (11)$$

The angle ω is thus greater than the angle ϕ by $1° \ 25'$[*].

Hence it follows that the principal edge is a *prominent* edge in the part K L, but a *receding* edge in the part J K.

We shall naturally find values for the angles ϕ, ψ, ω essentially different from the above when we employ a different unit in measuring either p or T in the construction of the temperature-surface. If, for instance, we choose one atmosphere as the unit of pressure, denoting by n the pressure in atmospheres, then we have

$$dp = 10333 \, dn, \quad . \quad . \quad . \quad . \quad . \quad . \quad (12)$$

and we obtain with this system of units the following equations for the angles:—

$$\tan \phi = \ \ 0\cdot000434, \text{ or } \phi = \ \ 0° \ \ 1' \ 30''; \quad . \quad . \quad (13)$$
$$\tan \psi = 133\cdot6, \qquad \text{ or } \psi = 89° \ 34'; \quad . \quad . \quad . \quad (14)$$
$$\tan \omega = \ \ 0\cdot00049, \text{ or } \omega = \ \ 0° \ \ 1' \ 41''. \quad . \quad . \quad (15)$$

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[*] Compare Kirchhoff, Pogg. *Ann.* ciii. p. 206.

§ 6. *Discontinuity on crossing the principal edge.*

In general we may represent a given change of state by the motion of a point along a definite line on the temperature-surface, and the law followed by the mass in changing state defines the form of the path-curve.

If at *every* point of the temperature-surface the state of the mass as well as the *position* of the point were defined without ambiguity when the values of the three coordinates were given, then might *every* line on the temperature-surface be considered as representing a *continuous* change of state, since to the passage along an infinitely small element of the path corresponds only infinitely small variations in the magnitudes that characterize the state of the mass. If the line were to cut an edge of the temperature-surface, it might still serve to represent a continuous change of state even at the points of intersection, although the law of change would in general undergo a sudden alteration as the edge was crossed.

By § 4, the *principal edge* represents along its whole length the singular cases of exception wherein the above condition is *not* satisfied. To every given point of this edge indeed correspond definite values of the coordinates p, v, T; but to each of these systems of values correspond an infinite number of different values of the intrinsic energy U. Here, therefore, a change of state can occur without any accompanying change of coordinates.

Considering the infinitely small line M N, cutting the principal edge, to represent an element of the path that represents the change of state, we see that in the passage from M to N the magnitude U jumps *discontinuously* from a greater to a smaller value, while the pressure, volume, and temperature change only infinitesimally (fig. 12). The initial point M just above the principal edge represents a mixture of *water* and steam, and the point N just below the edge a mixture of *ice* and steam. To each of these points corresponds a definite value of U; and the difference between these values is *finite*, reaching a maximum of more than 80 calories when the edge is crossed at the point K. To a *continuous* variation of U would correspond a discontinuous motion of the point representing the state of the mass. On arriving at the principal edge it would *suddenly* stop, remain there until the due variation of U was accomplished, and then continue its motion along a line lying at the other side of the edge.

To represent this discontinuity we may consider the temperature-surface cut along the whole length of the principal edge, and conceive this edge as a *double edge* made up of two

parallel edges lying infinitely near each other*. The two parallel edges may be considered as separated by a crevasse that reaches a maximum depth at K, gradually shallowing to zero towards J and L—the term depth not being taken in its actual sense, but figuratively [since there is no real variation of T].

§ 7. *Adiabatic passage over the principal edge.*

When a mixture of water and steam expands adiabatically, the temperature and pressure continuously diminish till the temperature falls to $t = 0°\cdot00744$. At this instant the water begins to freeze, and the heat that is thus set free prevents any further fall of temperature while any part of the mass remains liquid. The adiabatic will thus form an angular point at M where it reaches the principal edge, and its next piece will be a straight horizontal length coinciding with the principal edge (fig. 13). After the mixture of *water* and steam has been changed into a mixture of *ice* and steam, the temperature and pressure begin anew to fall. The extremity N of the horizontal length M N is therefore a second angular point on the adiabatic.

The motion of the point along the horizontal length M N represents a change of state during which part of the water freezes while another part vaporizes. The heat set free during the freezing of the first part goes to vaporize the second. If x_1 denotes the mass of steam in the condition M, and x_2 denotes the mass of steam at the end of the change M N, then $1 - x_2$ is the mass of water frozen and $x_2 - x_1$ the mass of water vaporized. The vaporization of the latter requires the heat

$$Q = r(x_2 - x_1); \quad . \quad . \quad . \quad . \quad . \quad . \quad (1)$$

and the heat set free during the freezing of the first part has the same value,

$$Q = l(1 - x_2). \quad . \quad . \quad . \quad . \quad . \quad . \quad (2)$$

On equating these two values and solving the resulting equation for x_2, we obtain

$$x_2 = \frac{l + r x_1}{l + r}. \quad . \quad . \quad . \quad . \quad . \quad (3)$$

The change of $x_2 - x_1$ kilograms of water into steam increases the volume by

$$v_2 - v_1 = u(x_2 - x_1); \quad . \quad . \quad . \quad . \quad (4)$$

and the work thus done by the steam-pressure is

$$\mathfrak{A} = pu(x_2 - x_1). \quad . \quad . \quad . \quad . \quad . \quad (5)$$

---

* C. Neumann, *Vorlesungen über die mechanische Wärmetheorie*, p. 159.

This work, which is represented in fig. 13 by the shaded rectangle, is equivalent to the heat

$$q = \mathrm{A} pu(x_2 - x_1). \quad . \quad . \quad . \quad . \quad . \quad (6)$$

Here $p = 62 \cdot 58$, $u = 210$, $l = 80$, $r = 606 \cdot 5$; and on substituting these values in the above equations we get them in the forms

$$x_2 = 0 \cdot 1165 + 0 \cdot 8835\, x_1, \quad . \quad . \quad . \quad . \quad (7)$$
$$r_2 - v_1 = 24 \cdot 5\,(1 - x_1), \quad . \quad . \quad . \quad . \quad . \quad (8)$$
$$q = 3 \cdot 61\,(1 - x_1). \quad . \quad . \quad . \quad . \quad . \quad (9)$$

If, for instance, the whole mass were at first liquid, so that $x_1 = 0$, we have in this case

$$x_2 = 0 \cdot 1165, \quad v_2 - v_1 = 24 \cdot 5, \quad g = 3 \cdot 61.$$

In the adiabatic change of a kilogram of water into a mixture of ice and steam, therefore, $0 \cdot 1165$ kilogram will vaporize and $0 \cdot 8835$ kilogram freeze; $3 \cdot 61$ calories of the intrinsic energy will be turned into external work; and the increment of volume represented in fig. 13 by the length M N will be $24 \cdot 5$ cubic metres.

§ 8. *Conclusion.*

As a result of the above discussion, there follows the *theoretical* possibility of representing the behaviour of water in its three different states of aggregation by a solid geometrical figure, even though considerable difficulties would be encountered in the exact *practical* execution of such a model by reason of the insufficiency of the experimental results already in hand[*].

The same procedure as this we have followed for water may also be adopted for representing the behaviour of any other body by a model of its temperature-surface. As the ground of such a model we may take a hyperbolic paraboloid, which represents the behaviour of the so-called perfect gases, and pieces may be stuck on it to represent the differing behaviour of other bodies.

On the model for such bodies as expand in melting (*e. g.* sulphur, phosphorus, &c.) the *frost-edge* will appear as a *receding* edge, and the *melting-edge* as a *prominent* one. Instead of the prominent cornice that represents the freezing-region on the water model, we shall find on the models for these bodies a *terrace-shaped* prominence to represent the same region.

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[*] A plaster model of the temperature-surface of water, made by the sculptor Blum, of Aachen, is in the museum of the Aachen Polytechnicum.

If the necessary data were known from experiment for all bodies, we might then exhibit their behaviour in changing state by a series of models—just as certain of their properties are naturally shown by their crystalline forms.

Aachen, June 28, 1877.

XXX. *On Unitation.*—VIII. *Practical Remarks thereon, together with Examples.* By W. H. WALENN, *Mem. Phys. Soc.*

[Continued from vol. iv. p. 379.]

29. IN the general formula for any integer number, given in art. 28, namely

$$a_n r^{n-1} + a_{n-1} r^{n-2} + \ldots + a_3 r^2 + a_2 r + a_1,$$

the suffixes to the coefficients which correspond to the digits are so disposed as to show the number of digits at a glance. They also show, by inspection, the place of any one digit in the number, counting the unit's digit as the 1st digit, symbolized by a_1, the tens' digit as the 2nd digit, symbolized by a_2, the hundreds' digit as the 3rd digit, symbolized by a_3, and so on. This use of the suffix implies a law; and the law is an extension upon that which has hitherto appeared in relation to suffixes ; this extension involves a special interpretation of the symbol a_0.

In this place it must be noted that suffixes have not been used with that attention to perfect congruity which should accompany every mathematical work. In ordinary algebra, these adjuncts to notation have most frequently been used for the *series of coefficients* in the general formula for an equation, or in an expression in which each term is presumed to have a coefficient, either known or unknown. In these cases, for the most part, the suffixes simply indicate the order in which the coefficients follow one another : sometimes this order is from the right hand to the left, and in opposition to the order of the indices of the powers of the unknown quantity or variable ; and sometimes it is in the same direction. A common use of the suffix is to mark the index of the power of the variable to which the coefficient belongs in any particular term. It is used in this way in Hind's 'Algebra' (second edition), chap. xi. p. 374, for instance in the formula

$$a_m r^m + \&c. + a_2 r^2 + a_1 r + a_0 r^0 + a_{-1} r^{-1} + a_{-2} r^{-2} + \&c.$$

Another use of the suffix is to mark the terms that disappear when a particular operation is performed upon a general ex-

pression ; this is the case in the formula

$$\sin\left(\frac{\pi}{2}x\mathrm{D}\right)\mathrm{X} = \mathrm{A}_1 x - \mathrm{A}_3 x^3 + \mathrm{A}_5 x^5 - \mathrm{A}_7 x^7 + \&\mathrm{c.,}$$

derived from

$$\sin\left(\frac{\pi}{2}x\mathrm{D}\right)\mathrm{X} = \sin\left(\frac{\pi}{2}.0\right)\mathrm{A}_0 + \sin\left(\frac{\pi}{2}.1\right)\mathrm{A}_1 x + \sin\left(\frac{\pi}{2}.2\right)\mathrm{A}_2 x^2 + \&\mathrm{c.}^*$$

But no full recognition of the function of a suffix appears in any of these uses. When this method of indicating the order of a series of quantities is completely developed, it should be capable of showing, not only the sequence and direction, but also the relative position of each member of the series to a given point, as in the case of the ordinal numbers, 1st, 2nd, 3rd, &c.

The use made of the suffix in unitation, as proposed in art. 28, is in accordance with these views ; and, in the general formula for integer numbers above cited, the suffix expresses the order of the digits commencing with the unit's digit, counting the unit's digit as the 1st integer digit from the decimal point, the tens' digit as the 2nd digit, &c. A question then arises which is as important, in relation to ordinal numbers, as the meaning of a^0 is in the theory of exponents. This question can be answered on a basis as logical as that of the exponential question : it is, " If the series be continued towards the right hand, as

$$a_n, \ a_{n-1}, \ a_{n-2}, \ a_{n-3}, \ \&\mathrm{c.} \ldots a_2, \ a_1, \ a_0, \ a_{-1}, \ a_{-2}, \ \&\mathrm{c.,}$$

what does a_0 mean ?" It is impossible to think of the next number to the right of the unit's digit (which is in the 1st decimal place) as having any relation to a_0 ; for as the unit's digit in the number is the 1st digit to the left of the decimal point, so the next figure to the unit's digit, on the right hand, is the 1st digit to the right of the decimal point. If these suffixes are to be read as ordinal numbers, a_0 must be left out ; for the 0th place to the right or left of the decimal point is the decimal point itself. That is, if the above general formula be extended to decimals (or towards the right hand), it must be written

$$a_n r^{n-1} + a_{n-1} r^{n-2} + \ldots + a_3 r^2 + a_2 r^1 + a_1 r^0 + a_{-1} r^{-1} + a_{-2} r^{-2} + \ldots$$

and a_0 has no other meaning than the decimal point itself.

Writing in the top row, which follows, an ordinary number with a finite number of decimal places, the corresponding co-

* See Dr. Graves (Bishop of Limerick), 'Law's Mathematical Prize,' 1853, quoted in Carmichael's 'Calculus of Operations,' p. 100.

efficients of the formula are represented in the underneath row
as follows :—

$$
\begin{array}{ccc|c|ccc}
7 & 4 & 5 & & 6 & 2 & 5 \\
a_3 & a_2 & a_1 & a_0\cdot & a_{-1} & a_{-2} & a_{-3}
\end{array}
$$

Thus written, each ordinal suffix has due relation to the posi-
tion of its corresponding digit, and the whole number, in
respect to the suffixes, is read:—5 is the 1st digit (from the
decimal point understood), 4 is the 2nd digit, 7 is the 3rd
digit.   Then (in the opposite direction) 6 is in the first de-
cimal place, 2 is in the 2nd, 5 is in the 3rd.

Normally considered, the operation of unitation always pro-
ceeds from right to left; but the negative suffix indicates the
possibility of a change from right to left to left to right, under
certain circumstances.   This view will receive further consi-
deration in the proper place.   The use of a_0 as determining
the place from which the direction of the operation is to be
reversed, is believed to be new, and may be of use in other
departments of mathematical science.   Thus the meaning of
a_0, in the series a_3, a_2, a_1, a_0, a_{-1}, a_{-2}, &c., is satisfactorily
made out, according to the principles of the interpretation of
symbols, to mean the place from which the order of the suf-
fixes is reckoned, in reference to direction of counting.

30. This interpretation of a_0 may be well illustrated by a
geometrical diagram :—If a vertical line cross a horizontal line,
as in the marginal dia-
gram, in the style of Des-
cartes's rectangular coor-
dinates, and if the origin
be taken as the point from
which the counting is to

$$a_n \cdots a_3\ a_2\ a_1\ a_0\ a_{-1}\ a_{-2} \cdots a_{-n}$$
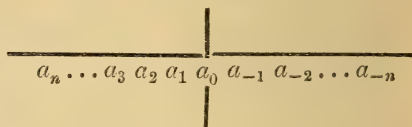
commence in both horizontal directions, namely backwards
and forwards, it is evident that the a which is at the origin is
a_0, and that the a's with the positive suffixes are distant from
the origin in the numerical order of their suffixes; also the a's
with the negative suffixes are distant from the origin in the
numerical order of their suffixes, the negative signs simply
indicating that the counting of the suffixes is to proceed in the
opposite direction to the counting of the positive suffixes.

31. The substitution of $(r-\delta)$ for r, in the formula

$$a_n r^{n-1} + a_{n-1} r^{n-2} + \ldots + a_3 r^2 + a_2 r + a_1$$

for a given number N, yields

$$a_n(r-\delta)^{n-1} + a_{n-1}(r-\delta)^{n-2} + \ldots + a_3(r-\delta)^2 + a_2(r-\delta) + a_1.$$

This is a number which has the same remainder to δ as N has;

for on expanding the $(r-\delta)^m$ portion of each term by the binomial theorem, it has δ as a factor in every term of the expansion (of any one term in the latter formula) except the first, which is the same power of r as occurs in the corresponding term of the original value of N.

32. In obtaining the remainder to δ of N, the formula in art. 31 may be extended, by means of negative suffixes, into

$$a_n(r-\delta)^{n-1}+a_{n-1}(r-\delta)^{n-2}+\ldots+a_3(r-\delta)^2+a_2(r-\delta)^1$$
$$+a_1(r-\delta)^0+a_{-1}(r-\delta)^{-1}+a_{-2}(r-\delta)^{-2}+\ldots,$$

thus making it available for other numbers than whole numbers. In the operation for obtaining the remainder, the number resulting from the first substitution of the digits in the formula is again subjected to the operation ; then this last number is again treated in the same way, and so on, each treatment giving a number less than the previous one, and divisible by δ with the same remainder that N has. If this treatment be continued until a number less than δ is obtained, that number is the unitate of N to the base δ. This is according to the definition of a unitate given in the Philosophical Magazine for November 1868, p. 346.

33. This method of obtaining the unitate of N is general, and is therefore valuable. It also affords a means of comparing the properties of $U_\delta N$ with those of N in a direct and satisfactory manner.

The repetition of the process of reduction by the formula is peculiar to unitation ; and it may be symbolized by $U_\delta^{(n)} N$, (n) being the number of times the formula is applied to a given determination of $U_\delta N$ in order that the ultimate value of $U_\delta N$ may be less than δ. This repetition has no analogy in the expression of a number by means of the formula N.

The following examples illustrate the repetition of the process of reduction :—

I. If N $=1234567$,

$U_9{}'$ N $=1+2+3+4+5+6+7=28.$

$U_9{}''$N $=2+8=10.$

$U_9{}'''$N $=1+0=1.$ Here $(n)=3.$

II. In obtaining $U_7 N$, if the formula containing the unreduced powers of 3 be used,

$U_7{}'$ N $=3^6.1+3^5.2+3^4.3+3^3.4+3^2.5+3.6+7=1636.$

$U_7{}''$ N $=3^3.1+3^2.6+3.3+6=96.$

$U_7{}'''$N $=3.9+6=33.$

U_7^{iv} N $=3.3+3=12.$

U_7^{v} N $=3.1+2=5.$ Here $(n)=5.$

III. If the formula with the reduced coefficients be employed[*], namely

$$U_7 N = a_7 + 5a_6 + 4a_5 + 6a_4 + 2a_3 + 3a_2 + a_1,$$

then

$$U_7' \ N = 1 + 10 + 12 + 24 + 10 + 18 + 7 = 82.$$

$$U_7'' \ N = 3 \cdot 8 + 2 = 26.$$

$$U_7''' N = 3 \cdot 2 + 6 = 12.$$

$$U_7^{iv} \ N = 3 \cdot 1 + 2 = 5. \quad \text{Here } (n) = 4.$$

34. In N, as soon as any value of a_n is increased, by the successive addition of units, up to or beyond r, it is transferred to the next higher term, or that containing the factor a_{n+1}, by adding a unit to the higher term and placing the remainder to r, or of the division $\frac{a_n}{r}$, in the term in which the lower factor a_n occurs; that is, r determines the maximum value of a_n in each term.

In $U_\delta N$, on the other hand, δ may be taken of any integer value in respect to r, and the formula will still be true, but r will have no power to determine the highest value of a_n in any term; δ is the only determinator of the maximum value of a_n in any term. For illustrations of this see Philosophical Magazine, May 1875, p. 347, and the above instances of $U_9 N$ and $U_7 N$.

35. The value of δ, whether integral or fractional, for instance, determines the degree and kind of discontinuity that exists in $U_\delta N$. For example, in $U_\frac{1}{2} 67 = \frac{1}{2}$, $\frac{1}{2}$ is taken, by inference, as the unit; the same occurs in $U_{9\frac{1}{2}} 67 = \frac{1}{2}$. In $U_{2\frac{1}{6}} 25 = \frac{7}{6} = 1\frac{1}{6}$, $\frac{1}{6}$ is the unit.

36. In regard to the arrangement of the terms, N gives simply the arrangement of a number in powers of r; whereas $U_\delta N$ gives the arrangement of the same number in powers of $(r - \delta)$. In the most useful form of $U_\delta N$, each power of $(r - \delta)$ is reduced by substituting for it its remainder to δ.

74 Brecknock Road, N.,
    December 1877.

* See Phil. Mag. May 1875, p. 347.

XXXI. *The Contact Theory of Voltaic Action. By* W. E. AYRTON *and* JOHN PERRY, *Professors of the Imperial College of Engineering, Tokio, Japan.*

To the Editors of the Philosophical Magazine and Journal.

The Imperial College of Engineering,
GENTLEMEN,   Tokio, Japan, December 14, 1877.

WHEN contributing his paper, "On the Difference of Potential produced by the Contact of Different Substances," to the Royal Society on May 22, 1877, Professor Clifton, of Oxford, seemed to be quite unaware of the elaborate series of experiments on exactly the same subject made by us in the winter of 1875, a full account of which was communicated in a paper on "The Contact Theory of Voltaic Action, Paper No. I.," to Professor Sir William Thomson, May 6, 1876, who at the British-Association Meeting at Glasgow of that year gave a public account of the method employed by us and the results we obtained, reserving our complete paper for the pages of the Proceedings of the Royal Society.

If the investigation in question had been of merely ordinary importance, we should not have deemed it necessary to point out the priority of our experiments to those of Professor Clifton; but when the fact is remembered (a fact not very evident from Professor Clifton's paper) that a series of experiments such as we performed clears up the long-standing discrepancies between the chemical and contact explanations of voltaic phenomena, and so is of extremely great importance in the science of energy, we trust we may be pardoned for claiming the priority due to us. Much of the ordinary original work performed in physical laboratories must, of course, be undertaken nearly simultaneously in different countries; and our great distance from Europe necessarily places us in the unfortunate position of being some months in time behind other men who publish papers in the same societies as ourselves; but in this particular case the work was not of an ordinary kind, and we have not to ask for the indulgence of scientific men in making allowance for our residence in Japan, seeing that, first, our paper reached England exactly one year before Professor Clifton's communication was made to the Royal Society, and, secondly, Sir W. Thomson was so kind as to give an account of our method and results to the British Association several months before Professor Clifton appears to have commenced his earliest experiments on the subject.

The method of experimenting employed by this gentleman is essentially the same as that used by ourselves, with this im-

portant difference, that whereas Professor Clifton only removes the plates of a condenser from a distance a apart to a distance b apart, we removed them to an infinite distance apart, and then put them in such a position that the original charge to be measured was doubled ; so that in fact our method was by far the most delicate, and was only limited in sensibility by the natural imperfections of mechanism. All this was clearly shown in the carefully executed drawing that accompanied our paper. The advantages we derived from the superior delicacy of our apparatus are seen if we examine, as may easily be done, the two papers paragraph by paragraph ; for the metals and liquids employed by Professor Clifton being the same as those used by ourselves, in every case that he in 1877 was only able to detect the difference of potentials, we in 1876 published not only the sign but also the numerical value of the difference in question (compare pages 301 to 305 of his paper in the 'Proceedings of the Royal Society,' No. 182, with our paper). Considering, too, that the quantities of electricity to be measured are so small, and consequently the slightest loss of electricity is so serious, we fail to see what benefit was derived from using six insulating stems instead of only the two carefully protected rods of our apparatus.

We observe that Professor Clifton assumes throughout his paper the " summation law of electromotive force," and that he was compelled to make such assumptions in consequence of his inability to measure *directly* with his apparatus the differ- ence of potentials between two *liquids* in contact. But if this be assumed, then we might have employed in our research the method of measuring the difference of potential of two liquids in contact that we have often, as early as 1874, employed as a lecture-illustration to indicate this difference. This method consisted in attaching to the terminals of a quadrant-electro- meter two platinum wires, of which the ends were respectively dipping into two liquids separated by a porous diaphragm ; but to make any use of the observations obtained from such an experiment, it must be assumed that the observed deflection of the electrometer represents the algebraic sum of the three contact differences of potentials such as might be measured separately. At first sight, not to assume this might appear to be a refinement of caution on our part ; but in reality it was imperative to prove experimentally that this assumption was true when it was taken in connexion with the statements ge- nerally made by the supporters of Thomson's theory of con- tact. For example, Professor Fleeming Jenkin says, on p. 44 of his ' Electricity and Magnetism':—" When a single metal is placed in contact with an electrolyte, a definite difference of

potentials is produced between the liquid and the metal. If zinc is plunged in water the zinc becomes negative, the water positive. Copper plunged in water also becomes negative, but much less so than zinc. If two metals be plunged in water (as copper and zinc), the copper, the zinc, and the water form- ing a galvanic cell, all remain at one potential, and no charge is observed in any part of the system." Consequently in 1875 we discarded our original proposed method of experimenting, which was to use an apparatus somewhat similar to that em- ployed by Professor Clifton, as far as we can understand it without a drawing; and we constructed the apparatus described in our paper, which enabled us to measure any single contact difference of potential, whether of a metal with a metal, or a metal with a liquid, or a liquid with a liquid, or a combination of any two or more contacts.

The very important fact that the rise of the difference of po- tentials between the plates of a voltaic cell on first immersion, when the circuit remains open, is due to the same cause as po- larization of the plates when the circuit is closed but operating in the opposite direction, as explained by Professor Clifton, was clearly stated by us in our paper in question under "the three states of a cell;" and our subsequent papers showed that we considered this effect to be analogous with the so-called soaking in and soaking out in any dielectric, or what is called the residual charge in a Leyden jar—a subject to which we have been since devoting much attention. But we even went further; for we found that even when the circuit was closed directly after immersion, there was first a rise of difference of potentials, followed afterwards by a fall; and this is an expla- nation of a want of constancy observed in many cells, and notably in the two-fluid cell described by Professor Clifton, page 309.

We take the liberty of observing that although a table of the difference of potentials of the terminals of different cells is of great value to practical men, still we should hardly have expected to find such a table at the end of Professor Clifton's paper with one number only (almost without exception) given for each cell, since he was quite aware that the difference of potentials between the electrodes alters from the first instant of immersion of the plates. Again, we do not understand how he can say that no current has passed; for it is evident that a current may pass without the electrodes being externally connected. A table such as is given by Professor Clifton would be very valuable if it gave the difference of potentials between the electrodes when the plates had been kept im- mersed for a sufficient length of time for the difference of po-

tentials to reach its maximum value; but it would be more valuable if it gave in addition the time-rise of the difference. We confess, however, that it is only with exceptional cells that we have succeeded in getting on different occasions exactly the same results with the same combination. Such a table as we suggest, which would be a great improvement on that given by Professor Clifton, could of course be constructed by any one possessing an electrometer without employing any special apparatus.

In conclusion we notice, page 299, that Professor Clifton sees the necessity of changing his apparatus, which could not measure directly the difference of potential between two liquids in contact, before he can obtain satisfactory measures of the difference of potential in certain important cases. We may mention that although the apparatus employed by us in our investigation described in our paper of 1876 enabled us to do this with considerable accuracy, still we thought it advisable, in the summer of that year, to construct a new apparatus, the accurate results obtainable with which will form the subject of our next paper on this subject.

<div style="text-align:center">We beg to remain, Gentlemen,</div>

<div style="text-align:right">Very truly yours,
W. E. Ayrton.
John Perry.</div>

XXXII. *On the Colour Relations of Copper and its Salts.* By Thomas Bayley, *Assoc. R.C.Sc.I.**

COPPER in solution, as is well known, imparts to the liquid a blue colour. In the case of the chloride the colour inclines to green, but becomes blue on dilution. Wishing to see what relation the light transmitted by such solutions bears to that reflected from the surface of the metal, I made the following experiments.

An extremely dilute solution of cupric sulphate having been prepared, it was placed in a glass tube closed at the end by a thin plate of glass similar to those used for covering objects under the microscope. The tube had a narrow side-tube near the bottom; this was fitted with a piece of caoutchouc tubing and pinchtap, so that any liquid contained in the tube could be drawn off. A flat plate of copper carefully polished, first with trent sand and oil and then with rotten-stone, was placed beneath the tube in such a manner that the diffused

<div style="text-align:center">* Communicated by the Author.</div>

daylight from a window was reflected from its surface verti-
cally upwards through the tube. The length of the column
of solution of cupric sulphate was then varied by letting out
small portions at a time through the side tube. It was found
that the plate of copper, viewed through a certain thickness
of the blue solution, appeared like a plate of polished silver.
This certainly tends to show that the colour transmitted by
solutions of copper is complementary to that reflected by the
metal.

I now placed a hollow glass prism before the slit of a spec-
troscope, in such a manner that the light passed through a
strong solution of cupric sulphate contained in it before reach-
ing the prism of the spectroscope. The accompanying sketch
(fig. 1) shows the effect of this upon the spectrum.

Fig. 1.

The part of the spectrum to the red side of the D line is more

Fig. 1 labels: Red. Yellow. Green. Blue. Violet. — Solar spectrum. — Solar spectrum after passing through copper solution. — Absorbed portion. D
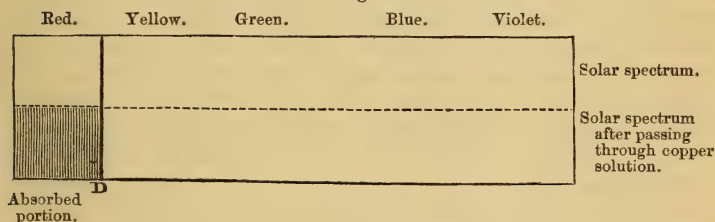
All light less refrangible than the sodium-line is very much
diminished by passing through the copper solution, while the
rest seems unaffected.

I next endeavoured to determine in what particulars the
light reflected from copper differs from ordinary light; and for
this purpose I compared the reflection from a polished sheet
of copper with that from a piece of white note-paper. The
result appears in fig. 2.

Fig. 2.

Fig. 2 labels: Red. Yellow. Green. Blue. Violet. — Intense Red — Spectrum of reflection from copper. — Spectrum of reflection from paper. D — Limit of solar spectrum.
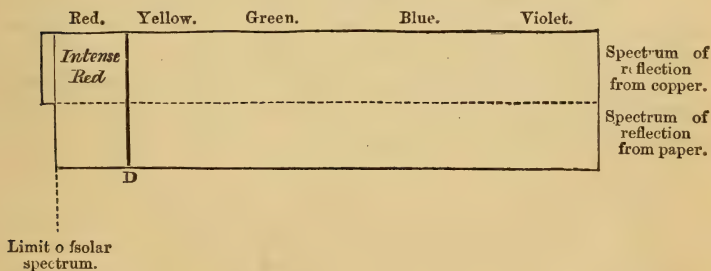
The part of the spectrum to the red side of the D line is more

intense in the light reflected from copper than in the light reflected from paper. The other parts of the spectra were of equal intensity in each. The red in the spectrum from copper is lengthened out beyond the point at which the red disappears in the spectrum obtained from white paper or direct from a window. As far as could be determined with the instrument at my disposal, the sodium-line exactly forms the boundary of the absorption-band of copper in solution, and of the bright-red region in the spectrum of light reflected from the metal.

The latter spectrum possesses all the characteristics of an ordinary one, with this exception, that its red region is intensified and somewhat lengthened out.

These results confirm the conclusions drawn from the experiments described as made with the tube. In those experiments the excess of red light was absorbed by the metal in solution and white light passed through. Several preliminary attempts to found a method of estimating copper upon these properties were made as follows :—Three tubes, similar to the first described, were placed parallel and vertical above a polished sheet of copper ; they were protected from extraneous light by a cylinder of blackened card, closed at the bottom by a piece of card also blackened, and pierced by three holes for the passage of the tubes. The tubes were graduated from the bottom upwards. In one tube a column of the dilute copper solution was placed of sufficient length to just allow the colour of copper to pass through, while the column in another tube was sufficiently long to cause a faint predominance of blue. A portion of the solution of unknown strength being placed in the third tube, it was easy to adjust its length until the light passing through it was intermediate between the red shade of the first tube and the blue of the second.

Other matters having intervened, I have been unable to proceed far in this direction ; but the results already obtained justify the expectation that the method would be of much value in approximately determining the strength of very dilute solutions of copper, such as those running from mines, from which the copper is precipitated by metallic iron.

These experiments were conducted in the chemical laboratory of the Royal College of Science, Dublin, by the kind permission of Professor Galloway.

XXXIII. *Notices respecting New Books.*

Photographed Spectra. One Hundred and Thirty-six Photographs of Metallic, Gaseous, and other Spectra, printed by the Permanent Autotype Process. By J. RAND CAPRON, *F.R.A.S.* London: E. and F. Spon.

THE work before us is a handsome octavo volume, consisting of 37 plates of permanent photographs and 80 pages descriptive of the plates. There are about 85 photographs of metallic spectra, and 50 of spectra of gases.

The metallic spectra extend mostly from H to a little beyond F, the red end of the spectra of course not being recorded by the photographic process. They were obtained with a direct-vision spectroscope with one compound prism of 5 prisms, and are thus spectra of small dispersion, the interval F to G occupying about $1\frac{1}{2}$ inch in the photographs. The author remarks that his results " are not intended to be placed by the side of photographs of spectra of larger dispersion taken for comparison of the metals, study of the solar spectrum, &c. ; but they may perhaps prove useful to amateurs and others working with spectroscopes of small dispersion, for comparison of spectra in their general aspect, and for study of the points and peculiarities attaching to most spectra which are generally brought out in our prints."

The spectra of the metals were partly obtained from the spark between points of the metal, and partly from ignition of pieces of the metal in the electric arc given by 40 pint Grove's cells.

The former series includes Arsenic, Aluminium, Antimony, Bismuth, Barium, Calcium, Cadmium, Copper, Indium, Iron, Lead, Magnesium, Mercury, Nickel, Palladium, Selenium, Silver, Strontium, Tellurium, Thallium, Titanium, Tin, Zinc, Zirconium. The spectra obtained from the voltaic arc (the more interesting series) are those of Aluminium, Antimony, Bismuth, Beryllium, Boron, Cadmium, Chromium, Cobalt, Copper, Didymium, Erbium, Gold, Indium, Iridium, Iron, Lead, Magnesium, Manganese, Molybdenum, Nickel, Niobium, Palladium, Platinum, Rhodium, Ruthenium, Silver, Thallium, Titanium, Tin, Tungsten, Uranium, Vanadium, Yttrium, Zinc, Zirconium.

Mr. Capron has earned the thanks of spectroscopists for the large amount of useful work which he has performed for them, in the way of preliminary investigation. No exact scale or measurements accompany the photographs ; and they will be chiefly useful in indicating by comparison with each other the lines which belong to particular metals, and the conditions under which particular lines are produced. In the investigation of the spectrum of any substance, it is a great saving of time to begin with small dispersion, and afterwards to apply higher powers when it becomes an object to determine exactly the wave-length of particular lines.

But although no exact measurements are given, and although the photographs vary very perceptibly in length, nearly all show lines

which can be used as reference-lines, by measurement from which the wave-lengths of the metal-lines can be determined.

Thus in all the spark-spectra the lines of air are present in large number throughout the spectrum.

The possessor of this book will find it an advantage to mark the wave-lengths of the lines in the air-spectrum, which can best be done on the enlarged photograph of the air-spectrum given in the extra plate at the end of the volume.

The line at the red end (marked "spark" in the scale on Plate I.) is the nitrogen double line $\left.\begin{array}{c} 5005 \\ 5002 \end{array}\right\}$. The next group in the large photograph is a group of nine lines, the third of which (the brightest) has the wave-length 4630, and the finer ones 4642, 4640, 4621, 4613, 4607, 4601, 4596, 4591. In the second photograph on Plate I. there are between these a group of three lines, 4803, 4788, 4779. The next conspicuous line in the large photograph is 4447, and then (with a faint band between) 4416. Then comes a broad mass of lines occupying some inch or so in length. The least-refrangible bright line of this group (not the fine one) is 4348. The centre of the broad band is 4230, then a very close pair of lines, 4190, 4184. Commencing now at the blue end of the spectrum, there is a solitary bright line 3995, then a hazy band the brightest part of which measures 4038, and then, easily recognizable in all the photographs, a double line (really triple) followed by a somewhat wider double, and then three equidistant lines with a still wider interval. These read :—

$$
\left.\begin{array}{c} 4069 \\ 4074 \end{array}\right\} \qquad \begin{array}{c} 4100 \\ 4104 \end{array} \text{ and } \begin{array}{c} 4123 \\ 4137 \\ 4149 \end{array}
$$
4076

This last is followed by the line 4155 ; and between this and the broad band 4230 is a fine double $\left.\begin{array}{c} 4190 \\ 4184 \end{array}\right\}$.

These air-lines are present in nearly all the spark-spectra (scarcely recognizable in the spectra of Barium and Strontium). The metal-lines may commonly be distinguished by their different character— for example, the Bismuth-lines 4392, 4259, 4560, and 4722, in the second photograph of Plate III., and the Lead-lines 4058, 4246, and 4385, in the first spectrum of Plate XI. The substances to which the lines are due must be decided by internal evidence; for there is no evidence given of the chemical purity of the metal employed, and there is frequent evidence of impurity : for example, the Ruthenium-spectrum is chiefly due to iron.

The value of the book would be much increased by a systematic identification of the lines—a work which the author does not seem to have very carefully attempted; at least he remarks that the Cadmium spark-spectrum is "clear of air-lines," whereas the fact is that out of about 21 lines all but 2 are due to air. By such a systematic investigation, the spectra of many elements which have been pretty carefully investigated might be extended considerably towards the blue end with a certain amount of precision.

The advantages of the photographic method are noticed by the author as follows :—"Absolute truth is everything in spectroscopic work ; and the very best draughtsman working with the most perfect micrometer cannot, even at the expense of a vast amount of labour, equal in accuracy a good photograph of a set of spectral lines." Of course the photographic plate cannot miss or make mistakes in lines actually presented to it ; and Mr. Capron's work, as already remarked, is very useful as preliminary ; but the accuracy obtainable by measurement of his photographs is not as great as the author supposes. Indeed a greater degree of accuracy is attained by eye-measurements with a good micrometer. For measurement of the photographs, the reviewer has employed a photographic reduction (on glass) of a millimetre-scale having about 5 divisions to a millimetre. For example, the wave-lengths of the Aluminium-lines obtained from an interpolation-curve drawn from the air-lines gave 5047, 4660, 4528, 4510, and 4476 ; Thalén has for the same lines 5056, 4662, 4529, 4511, and 4476. And similar reductions of the iron-lines in the selenium-spark gave 4416, 4383, 4407, 4323, 4304, 4268, 4257, 4248, 4148, 4072, 4066, and 4048, where Thalén has 4415, 4383, 4404, 4325, 4307, 4271, 4260, 4251, 4143, 4071, 4063, and 4045. With a similar direct-vision spectroscope, and the micrometer described in the Number of this Journal for August 1875, closer measurements can be obtained, as is seen by the following comparison :—

Lines in the Spectrum of burning Magnesium.

Direct-vision spectroscope.		Six-prism automatic spectroscope.
5006	....	5007
4996	....	4997
4985·5	....	4986
4974·5	....	4975
4963·5	....	4963
4948·5	....	4948
4934	....	4934

Still very respectable results may be obtained by careful measurement of these photographs.

There are several points of interest suggested by a study of these " photographed spectra,"—why, for example, some lines of particular metals are plainly marked, while others are absent. For instance, in the Lead spark-spectrum the line 4058 is sharp and bright, while there is no trace of the brighter line 4167.

The photographs obtained from the electric light are particularly interesting. This is, as far as we know, the first extensive series of measurements of spectra obtained by the ignition of substances in the electric arc. In all the photographs are seen, more or less distinctly, certain lines which Mr. Capron terms " point-lines." A few of these are certainly Iron-lines ; but a particular set, employed by Mr. Capron as "arc" reference-lines, are certainly due to Carbon. They are seen in the third photograph of Plate V., and are the

lines which constitute the group θ of the Carbon-spectrum de-
scribed in this Journal, S. 4. vol. xxxviii. p. 249. They are known
to be due to Carbon and not to any of its compounds, inasmuch as
they are given by the spark of the induction-coil in Carbonic oxide
at high pressure, or in vacuum-tubes enclosing Cyanogen or Naph-
thaline; and evidence is given in the paper cited that they are
caused by Carbon at a higher temperature than that required for
the production of the ordinary spectrum. A curious point noted
by Mr. Capron in connexion with these lines is, that they are
scarcely seen in the spectrum of the arc between carbon poles, but
are brought out prominently on the addition of a volatile metal.
They are very well seen in the photograph of the Cadmium-spectrum;
and Sodium (which Mr. Capron does not appear to have tried)
is still more active in bringing them out. If they are due to incan-
descent carbon-*vapour*, the existence of the vapour for a moment
may be intelligible in view of the strong reducing-powers of Sodium
and Cadmium.

Other Worlds than Ours. By R. A. Proctor. Fourth Edition.
Longmans and Co. 1878.

The announcement of a fourth edition of 'Other Worlds than
Ours' is a guarantee of the estimation in which Mr. Proctor is
held as a popular scientific writer, particularly on astronomical
subjects. In the present work his fertile pen attains a more
lofty theme, soars above the passive material of suns, planets,
and stars, and seeks in primary and secondary systems the abodes
of living and intelligent creatures: accordingly we find it interme-
diate between an astronomical and biological treatise; dealing on
the one hand more with the *conditions* of life than with life itself,
while on the other such astronomical facts only are presented to
the reader as the author considered necessary to illustrate and sup-
port his main subject.

The conditions of life on the Earth is the first lesson in connexion
with other worlds taught us by the Solar system; the distribution
of climates, the adaptability of various forms of life to each, the
regions capable of supporting certain kinds of vegetable and animal
existences while others are totally unfit for maintaining these par-
ticular forms, are arguments used by the author in treating of those
globes in the Solar system which, from astronomical and meteoro-
logical considerations he regards as suitable habitations for intelli-
gent beings.

The great reservoir of living force Mr. Proctor finds in the Sun,
the central and ruling body of the system. The light, heat, actinism,
magnetism, and other influences emanating from him are trans-
mitted to the globes around him, which respond, as in the case of
magnetism, to the disturbances set up in the ocean of light sur-
rounding him. The remarkable connexion existing between those
tumultuously rushing currents rending apart the luminous clouds
constituting his photosphere, and the delicate vibrations of the
magnetic needle on the earth, point to a bond of sympathy between

the two bodies, the enunciation of which is eloquently treated by our author; and no less interestingly is the maintenance of these mighty forces presented to the reader in the chapter on Meteors and Comets, which strongly reminds us of Haidinger's theory of the formation of the Solar system from the aggregation of " cosmical dust."

In treating of the special object of his work, the author calls attention to the division of the orbs of the Solar system into two classes, those of the minor and major planets, those nearer and those further from the Sun; and he finds that among the nearer orbs the conditions of *life* obtain to the greatest extent, while among the four larger planets the conditions which he is able to detect are incompatible with life such as we find on our own planetary abode— but rather that the two which are most open to our scrutiny have formerly borne, and may still to a certain extent bear, the relation of suns to the systems of moons circulating around them, which he considers may in all probability be so constituted as to sustain life such as we are acquainted with.

Having expressed in the fullest manner his views of the habita- bility or otherwise of Solar orbs, Mr. Proctor passes on to consider the question, Are the multitude of Stars which surround us Suns similar to our own? In treating of these bodies he divides them also into two classes—one consisting of those in which the spectro- scope reveals the existence of elements familiar to dwellers on the Earth, the other of those which the author terms " minor stars," and which he considers are situated *among* the lucid stars—the two classes, with the Nebulæ, constituting one great system, the outer boundaries of which our most powerful telescopes are quite unable to reach. Reasoning from the analogy of the Solar system, Mr. Proctor suggests that the larger stars, some of which are consider- ably larger than our sun, are surrounded by worlds of a similar character to our own. The following quotation, which closes the chapter on the Sun, embodies an epitome of his views :—

" Lastly, turning from our sun to the other suns which shine in uncounted myriads throughout space, we see the same processes at work upon them all. Each star-sun has its coronal and its zodiacal disks formed by meteoric and cometic systems ; for otherwise each would quickly cease to be a sun. Each star-sun emits, no doubt, the same magnetic influences which give to the zodiacal light and to the solar corona their peculiar characteristics. Thus the worlds which circle around those orbs may resemble our own in all those relations which we refer to terrestrial magnetism, as well as in the circumstance that on them also there must be, as on our own earth, a continual downfall of minute meteors. In those worlds, per- chance, the magnetic compass directs the traveller over desert wastes and trackless oceans ; in their skies, the aurora displays its brilliant streamers ; while, amid the constellations which deck their heavens, meteors sweep suddenly into view, and comets extend their vast length athwart the celestial vault, a terror to millions, but a subject of study and research to the thoughtful."

A Treatise on the Stability of a given State of Motion, particularly steady Motion; being the Essay to which the Adams Prize was adjudged in 1877, in the University of Cambridge. By E. J. ROUTH, M.A., F.R.S., &c. London: Macmillan and Co., 1877. (8vo. Pp. 108).

The question, to which this Essay is an answer, was proposed in the following words :—" The Examiners give notice that the following is the subject of the Prize to be adjudged in 1877 : *The Criterion of Dynamical Stability.* To illustrate the meaning of the question, imagine a particle to slide down inside a smooth inclined cylinder along the lowest generating line, or to slide down outside along the highest generating line. In the former case a slight derangement of the motion would merely cause the particle to oscillate about the generating line, while in the latter case the particle would depart from the generating line altogether. The motion in the former case would be, in the sense of the question, stable, in the latter unstable. The criterion of the stability of the equilibrium of a system is, that its potential energy should be a minimum ; what is desired is a corresponding condition enabling us to decide when a dynamically possible motion of a system is such, that if slightly deranged, the motion shall continue to be only slightly departed from."

In very brief outline Mr. Routh's answer to the question is as follows :—When a dynamical system is making small oscillations under the action of any forces which may or may not possess a force-function, and is subject to resistances which vary as the velocities of the parts resisted, the general equations of motion are linear; and if $x = \mathrm{M}e^{mt}$, &c. their solution depends on a determinantal equation

$$f(m) = \begin{vmatrix} \mathrm{A}, & \mathrm{B}, & \mathrm{C}, & \ldots \\ \mathrm{A}', & \mathrm{B}', & \mathrm{C}', & \ldots \\ \cdot & \cdot & \cdot & \cdot & \cdot & \cdot \end{vmatrix} = 0,$$

the constituents being all of the form $\mathrm{A} = \mathrm{A}_2 m^2 + \mathrm{A}_1 m + \mathrm{A}_0$; and if the system has n degrees of freedom, $f(m)$ is of the order $2n$. " If the roots of this equation are all unequal, the motion will be stable if the real roots and the real parts of the imaginary roots are all negative or zero, and unstable if any one is positive. If several roots are equal, the motion will be stable if the real parts of those roots are negative and not very small, and unstable if they are negative and small, zero, or any positive quantity. But if, as often happens in dynamical problems, the terms which contain t as a factor are absent from the solution, the condition of stability is that the real roots and real parts of the imaginary roots of the subsidiary equation should be negative or zero." (P. 10.)

When the system has two degrees of freedom the equation $f(m) = 0$ is biquadratic ; and this case has been worked out completely in the third edition of the Author's treatise on Rigid Dynamics (pp. 345, ...). In the present case about a third of the Essay is devoted to a consideration of methods by which, without

solving the equation, it can be determined whether the real roots and the real parts of the imaginary roots be negative.

When the question has been treated thus far, a number of subsidiary points come under notice : *e. g.* in the case in which the system has a force-function, the equation $f(m)=0$ contains only even powers of m. This case also presents several points for discussion, such as the difference between oscillations about a position of equilibrium, and about a position of steady motion. Amongst other points which come up for discussion we will instance one more, to which a separate chapter is devoted, viz. the question under what circumstances it is necessary to examine the terms of the second order in order to assure ourselves of the stability of the motion ; for it is possible that some of these may have their periods so timed that their effects accumulate until the character of the motion is changed.

It is well known that, though it was intended to be given every alternate year, the Adams prize is but rarely awarded. We believe that on the occasion of Mr. Routh's Essay the award was made for only the fifth time since the year 1848. This is a fact which renders any praise of ours superfluous.

XXXIV. *Proceedings of Learned Societies.*

GEOLOGICAL SOCIETY.

December 5, 1877.—Prof. P. Martin Duncan, M.B., F.R.S., President, in the Chair.

[Continued from p. 158.]

THE following communications were read :—

3. " On some Precambrian (Dimetian and Pebidian) Rocks in Caernarvonshire." By Henry Hicks, Esq., F.G.S.

In this paper the author gave an account of the special examination of the great ribs of so-called intrusive felspathic and quartz porphyries which are found associated with the Cambrian rocks in Caernarvonshire, made by him in company with Prof. Hughes, Mr. Hudleston, and Mr. Homfray last summer. He described sections at and near Moel Tryfan and across the mass from Pen-y-groes to Talysarn, in which he showed that instead of being of an intrusive nature, as hitherto supposed, the whole, with the exception of a few dykes at those parts, is made up of bedded volcanic rocks, lavas, breccias, &c., similar to those found in the Pebidian series at St. David's, and that the Cambrian rocks, instead of being intruded by this mass, rest everywhere upon it unconformably, and the pebbles in the conglomerate of the Cambrian at the base are, as at St. David's, identical with, and must have been derived from the rocks below. Similar results were obtained in the examination to the north and south of Llyn Padarn ; and the conclusion, therefore, at which the author has arrived with regard to the great mass which

extends from Llanellyfine in the south to St. Ann's chapel in the north is that it is entirely Precambrian, and that it belongs to the series described by him under the name Pebidian at St. David's.

The other mass, extending from Caernarvon to Bangor, he considered also entirely Precambrian; and from the mineral characters exhibited by a portion of this mass directly behind Caernarvon, he thought it would prove to be, at least at this part, of Dimetian age. The altered beds near Bangor and their associated quartz felsites he considered entirely of Pebidian age, as there is no evidence that the Dimetian rocks are exposed there.

4. "On the Precambrian Rocks of Bangor." By Prof. T. McKenny Hughes, M.A., F.G.S.

The author described a series of slates, agglomerates, and porphyritic rocks which, near Bangor, are seen to pass under the Cambrian and seem to rest conformably upon the quartz felsites and granitoid rocks of Caernarvon. He thought that the Bangor beds were the equivalents of the felsitic and porphyritic series of Llyn Padarn; and, in order to bring his interpretation into harmony with the observations of Prof. Ramsay, he explained away the apparent melting of the ends of the Cambrian beds in that section by twists, faults, and dykes. He referred the apparent unconformity recorded by Mr. Maw entirely to rock-structure, produced by cleavage on beds of different texture.

He considered that in the main the Bangor beds were the equivalents of the Pebidian of Dr. Hicks, while the Caernarvon beds nearly represented his Dimetian. But he thought there was as yet no proof of an unconformity between these formations. He would explain the apparent unconformity at St. David's by a continuation of bends and faults and joints mistaken for bedding, and would refer the brecciated rock of Low Moor, near St. David's, to the Pebidian, thus taking it on the wrong side of the supposed unconformity. He thought that the green beds in the Dimetian were, in all the cases where he had been able to examine them, originally dykes.

He saw, therefore, no reason from an examination of other areas to suspect any different explanation from that suggested by the examination of the Bangor and Caernarvon district, viz. that we have in the Bangor and Caernarvon beds one great volcanic series, on which the Cambrian conglomerates and grits rest with a probable unconformability.

An appendix by Prof. Bonney on the microscopical examination of the rocks referred to accompanied this paper.

December 19.—Prof. P. Martin Duncan, M.B., F.R.S., President, in the Chair.

The following communications were read:—

1. "On *Argillornis longipennis*, Owen, a large bird of flight, from the Eocene Clay of Sheppey." By Prof. Owen, C.B., F.R.S., F.G.S., &c.

2. " Contributions to the history of the Deer of the European Miocene and Pliocene Strata." By Prof. W. Boyd Dawkins, M.A., F.R.S., F.G.S.

3. " On the occurrence of *Branchipus* (or *Chirocephalus*) in a fossil state, associated with *Archæoniscus*, and with numerous Insect-remains in the Eocene Freshwater Limestone of Gurnet Bay, Isle of Wight." By Henry Woodward, Esq., F.R.S., F.G.S.

4. " The Chronological Value of the Pleistocene Deposits of Devon." By W. A. E. Ussher, Esq., F.G.S., of H.M. Geological Survey.

In this paper the author endeavoured to work out the sequence of events indicated by the Pleistocene deposits of Devonshire. He believed that during late Tertiary times subsidence extended to the south-western counties ; and to this he ascribed with some doubt the accumulation of a patch of gravel on the north summit of the Black Downs and of part of the old bone-breccia of Kent's Cavern. In the Glacial period, with the increase of cold, snow accumulated on the high lands, with formation of glaciers, which descended and united to form a great ice-field, planing the surface of a district composed chiefly of Cretaceous and probably Tertiary strata. To this period the author ascribed the formation of the clay with unworn fragments of flint and chert, and, doubtfully, part of the clays of the Bovey valley, the clay of Petrockstow, and part of the bone-breccia and the crystalline stalagmite of Kent's Cavern. The Postglacial phenomena he referred to three subperiods, in the first of which, during a gradual amelioration of the climate and disappear-ance of the ice, large quantities of surface-water were set free, redistributing and removing Tertiary outliers, partially destroy-ing the old ice-beds and moraine rubbish, and sweeping Secondary deposits from Palæozoic districts. The deposits then formed were supposed to be the old gravel patches of Colford and Orleigh Court, the waterworn materials on the Blackdowns and Haldon, the sands flanking the Bovey valley, and, with doubt, the redistributed Triassic pebble-beds of Straightway Hill, and part of the cave-earth of Kent's Cavern. The next subperiod he regarded as one of great fluviatile action, the land being higher than at present, though sinking, and the meteorological conditions such as to greatly increase the volume of the rivers. The subsidence having continued to the level of the present raised beaches, reelevation took place, producing greater cold and more extreme seasons, and culminating in the production of continental conditions, permitting the southward mi-gration of a temperate fauna, and the advent of one requiring greater cold. During this period the gravels connected with the formation of the present valley-system, the raised beaches, and the " Head " were produced, and, doubtfully, part of the cave-earth and the granular stalagmite of Kent's Cavern, and the clay of Petrock-stow and Roundswell. In the last subperiod the author considered that a subsidence took place, during which most of the valleys were

excavated to their present depth, and forest-growth took place upon the old marine plain. The forests were then gradually circumscribed by the encroaching sea and diminishing rainfall, which also led to changes in the streams; and finally the sea entombed the forests and swamps on the coasts, and produced the present cliff-line. The results of this period are the submarine forests, most of the river-valley gravels, and alluvial tracts bordering the present river-courses.

January 9, 1878.—Prof. P. Martin Duncan, M.B., F.R.S.,
President, in the Chair.

The following communications were read :—

1. " On the Great Flat Lode south of Redruth and Camborne."
By Dr. C. Le Neve Foster, B.A., F.G.S.

The author described the mode of occurrence of the stanniferous deposit known as the Great Flat Lode, the mines worked in which extend for a distance of $3\frac{1}{2}$ miles, and furnish about one eighth of all the tin raised in Cornwall. The mines in question are Wheal Uny, South Carn Brea, West Wheal Basset, South and West Wheal Frances, South Condurrow, and Wheal Grenville ; and in all the lode dips at a much less angle than the average of Cornish veins, the dip at Wheal Uny being only about 46° S. Throughout the lode contains a small leader, usually only a few inches wide, occupying the space due to the shifting of the two sides of a fissure, and filled partly mechanically, partly chemically. Above, or below, or on both sides of this there is a mass of stanniferous schorl rock from 4 to 15 feet wide; this contains from 1 to 3 per cent. of cassiterite, in little grains, or in strings or veins. Schorl rock, very poor in tin (locally called *capel* or *greyback*), separates the lode from the surrounding granite or killas, but passes on one side into the lode, and on the other into the granite or killas, so that no *wall* is recognizable. From these characters the author inferred that the lode and the capel are merely altered rocks, the fissure now occupied by the leader having served to bring up vapours or solutions which have entirely changed the rocks on both sides of it. In support of his opinion, the author adduced other instances of the change of both granite and killas into schorl rock; and further stated that, both at South Condurrow and Wheal Grenville, he has found in the schorl rock cavities as large as a pea, agreeing in form with crystals of orthoclase felspar.

2. " On some Tin-mines in the Parish of Wendron, Cornwall."
By Dr. C. Le Neve Foster, B.A., F.G.S.

The mines described in this paper are called Balmynheer, The Lovell, and South Wendron. In the former the stanniferous de-posit consists of a large irregular mass of rock 30–50 feet thick ; its dip is N., at an angle of about 30°, and its strike E. 32° N., along which it has been traced for 36 fathoms. The tinny rock is sepa-rated from the granite above by a slide or vein of white clay, with

a little quartz and mica, about 6 inches thick, but passes insensibly into the granite below. At the Lovell Mine there are two lodes, north and south, the former striking from 37° to 45° N. of E. and dipping N.W. at an angle of about 70°, the latter running E. 48° N. and dipping N.N.W. about 60°, so that the two lodes unite in going eastward and in depth. The lode is separated on one or both sides from the adjoining granite by a rock locally known as " cab," 6–12 inches thick, composed of quartz, mica, gilbertite, chlorite, iron-pyrites, copper-pyrites, and a little schorl. The lode itself shows joints which are mere planes of division in the rock, and usually have the same strike and dip. Divergent joints also occur ; and where these traverse the granite they carry with them a little tin-stuff for some distance. The South-Wendron Mine is worked in an irregularly cylindroid pipe of tinny rock, merging gradually on all sides into the granite ; the shorter axis of its oval section is about 10 feet, while the longer axis varies from 20 to 60 feet. It dips at an angle of 49° in a direction N. 25° W. The stanniferous rock in these mines is essentially a mixture of quartz, chlorite, gilbertite, iron-pyrites, and tin-ore, with zinc-blende in some cases, and usually some mica ; fine needles of tourmaline occur in the cavities which it encloses. In the South-Wendron Mine the southern part of the pipe is sometimes very granite-like in appearance, consisting of pink orthoclase crystals imbedded in a mass of quartz, chlorite, mica, and iron-pyrites, with a little copper-pyrites, fluor, and tin-ore. One specimen is a true stanniferous granite. These characters lead the author to the same conclusion he has arrived at in the case of the Great Flat Lode, namely that these tin deposits consist entirely of altered granite, and are not ordinary mineral veins : they have no walls, but the stanniferous rock passes gradually into granite ; and they show no signs of banded structure due to the successive deposition of minerals. The highly granitic character of part of the South-Wendron tin deposit is strongly confirmatory of this view, which is further supported by the occurrence, in the dark mass of the so-called lode at the Lovell, of pseudomorphs of quartz after orthoclase containing a little cassiterite.

3. " On some of the Stockworks of Cornwall." By Dr. C. Le Neve Foster, B.A., F.G.S.

The author commenced by explaining that the term " Stockwork " had been derived from the German *Stockwerck*, meaning " Storywork," in allusion to the method of working in steps or stories in open workings, originally adopted for such deposits. Their being worked in open quarries affords a good opportunity of studying the mode of occurrence of tin ; and many of them are interesting on account of the small percentage of tin which will cover all expenses. Thus, in the Wheal-Prosper Mine, the average amount of oxide of tin obtained per ton of stuff is not more than 3 lb., worth, at the present price of " black tin," 4½d. per lb. ; so that the ground as it stands is only worth 13½d. per ton. The mine can be worked without loss, on account of the softness of the rock and the large size

of the grains of tin-ore, the comparative lightness of the substances associated with it, and the command of water-power.

The deposits worked as stockworks occur in Cornwall in killas, granite, and elvans. The tin-ore, associated with quartz and with small quantities of other minerals, is found in more or less parallel thin veins and strings, dipping at a high angle, and occasionally giving off branches or uniting with one another both in dip and strike. In the killas the rock close to the veins is occasionally altered into tourmaline-schist; in the granite the walls of the veins, and sometimes the whole mass of granite, are altered into greisen and schorl rock. At Carclaze the orthoclase of the intervening bands of granite has been converted into china-clay, which is now the main object of the working. At Carrigan the leader sometimes adheres to the enclosing rock by one side only, the other being bounded by a clay vein which contains broken crystals of cassiterite, indicating, in the author's opinion, that a movement of the walls has taken place since the deposition of the tin-ore. Of the stockworks in elvans the author gave a list, and remarked that the elvan of the Terras Mine is particularly interesting, as it presents a series of cavities left by the removal of orthoclase, and now being filled up with schorl and a little oxide of tin.

4. "The Precarboniferous Rocks of Charnwood Forest.—Part II." By the Rev. E. Hill, F.G.S., Fellow and Tutor, and the Rev. T. G. Bonney, F.G.S., Fellow and late Tutor of St. John's College, Cambridge.

The authors described the result of the microscopic examination of a considerable series of the clastic rocks of Charnwood. Many of these, even among the finer beds, prove to be of pyroclastic origin. The coarser are generally composed of a groundmass of pulverized felspar, with viridite and some iron peroxide, full of larger fragments of felspar crystals (generally both of orthoclase and plagioclase) and lapilli. The structure of these is often distinct, some are certainly andesites, others some kind of trachyte; slaty fragments are also present, and occasional grains of quartz. The authors express their opinion that all the larger felspar crystals, and most, if not all, the quartz grains, are of clastic origin, even in the more highly altered varieties. Some of the larger fragments in the breccias were examined, and referred in part to devitrified trachytes not very rich in silica. The igneous rocks were then described. The syenites of the southern and northern districts were shown probably to belong to one system of intrusion. The hornblendic granite of the Quornden district was also described, and the microscopic structure of the different varieties of it and the above investigated. A number of igneous rocks generally forming dykes in these was described: some appear to be altered basalts, others andesites; one is a felsite, another a diorite. A group of outlying igneous rocks in the vicinity of Narborough was described. Of these, one is a quartz felsite with some hornblende; another varies between this and a

quartziferous syenite; the rest are syenites; and one contains so much plagioclase as to be almost a diorite. One of the above, near Enderby, is seen to be distinctly intrusive in an altered slaty rock, which the authors have no doubt belongs to the Forest series. This discovery proves the igneous character of these rocks also, and extends the area of the slaty series 5 miles further south than was previously known. A section was devoted to the faults of the Forest region. Here the principal fault runs along the anticlinal axis, with a downthrow on its eastern side which diminishes from 2500 feet at the north end to 500 feet at the south end. East of this the beds seem undisturbed; but on the west they are shattered by many faults, whose course cannot be traced. These are most numerous near Whitwick. The anticlinal fault is Precarboniferous.

In conclusion, the age of the clastic and of the igneous rocks was discussed. The authors inclined to the opinion that the former are of the same age as the Borrowdale series of the Lake district (Lower Silurian), but admitted that the recent discovery of agglomerates in the Precambrian rocks of Wales, and in the probably Precambrian ridges of the Wrekin district, weakens the arguments for this correlation. They do not think that there is any reason for supposing them Cambrian. If the Charnwood series is Lower Silurian, they think it most probable that the syenites and the Quornden granite were intruded in some part of the Old-Red-Sandstone period, and that the later dykes were very probably Postcarboniferous but Pretriassic.

XXXV. *Intelligence and Miscellaneous Articles.*

ON SOME MEASUREMENTS OF THE POLARIZATION OF THE LIGHT
   COMING FROM THE MOON AND FROM THE PLANET VENUS. BY
   THE EARL OF ROSSE, F.R.S.[*]

SEVERAL years ago, at the suggestion of a friend, having examined some portions of the lunar surface with a Nicol's prism with a view to the detection of small sheets of standing water, if any such chanced to exist, I was led on to make a rather extended examination of particular portions of the surface with the polarimeter, under the idea that if the precise position of elongation from the sun where the polarization of a point of the lunar surface attains a maximum could be accurately determined, it might be possible to obtain an approximate value of the refractive index of the material composing that surface, and so to distinguish between material of a vitreous nature, ejected from volcanoes, and a surface of ice and snow.

The subject has been invested with the greater interest from the fact that Arago, having found the maximum of polarization of the whole of the moon's light to occur at or near quadrature,

[*] From the Proceedings of the Royal Dublin Society, May 21, 1877.
Communicated by the Author.

remarks on the circumstance as being what might be expected to result from the reflecting surface being gaseous; and he appears to think that the polarimeter may afford us some information on the question of the existence of a lunar atmosphere[*].

During the years 1872–75 we have at intervals made a rather extended series of measurements with the polarimeter, of which several different forms were tried, but one differing in little from Arago's was found the more satisfactory.

Although from a series of sixteen readings of the inclination of the plates of parallel glass a value may be obtained for the polarization on each night with a probable error of observation not exceeding 1 per cent., from some cause not yet established the discrepancies between the various nights' work are much larger, and the results must be accepted with reserve and regarded as only provisional.

The most probable values for the polarization (P), meaning by that term the proportion between the intensities of the components of the light polarized in and perpendicular to the plane passing through the sun at the several elongations (E), are for Mare Crisium—

E=	P=1÷	E=	P=1÷
60°	0·830	110°	0 840
70	0·815	120	0·890
80	0·795	130	0·930
90	0·785	140	0·965
100	0·805	150	0·980

Similar but less numerous measures than those on which the above table is based were made for Mare Imbrium, Mare Serenitatis, Palus Somnii, and the region between Macrobius and Proclus and other parts. The polarization varies with the situation and with the nature of the surface, being in general greater on the plains than on the more uneven parts.

Measurements of the light of the planet Venus made between 1872, March 12 and April 6, gave a mean value for the polarization of 0·925, of which no regular variation was perceived during the progress of the observations, although the change of phase which occurred during the interval was considerable.

May 15, 1877.

GLASS-ENGRAVING BY ELECTRICITY. BY M PLANTÉ.

I have previously described an experiment in which a glass tube, through which passes a platinum wire serving as electrode to a powerful galvanic current, was found to be instantaneously hollowed out in a conical or tunnel-shape within a voltameter containing a saline solution. In other experiments, on the luminous effects pro-

* Arago, *Œuvres*, nouv. éd. par Barral, livr. xiv. chap. vi., t. ii. p. 101 &c.

duced by a current of strong tension, at the contact of the positive or negative electrode with the sides of a glass or rock-crystal vessel moistened with a solution of sea-salt, I observed that the glass or crystal was powerfully attacked at the points touched by the electrode, and that the concentric luminous rings formed around remained sometimes engraved at the surface of the glass of the voltameter. On employing nitrate of potash as the saline solution, much less electric force was required, than with chloride of sodium, to produce these effects.

These observations led me to apply the electric current to engrave on glass or rock-crystal. The surface of a plate of glass or crystal is covered with a concentrated solution of nitrate of potass by simply pouring the liquid on the plate placed horizontally on a table or in a shallow basin. Next, a horizontal platinum wire, connected with the poles of a secondary battery of from 50 to 60 elements, is immersed in the layer of liquid which covers the glass, along the edges of the plate; then, holding in his hand the other electrode, consisting of a platinum wire enclosed, except at its extremity, in an insulating sheath, the operator touches the glass, covered with the thin layer of saline solution, at the points where he wishes to engrave characters or a design.

A luminous trail is produced wherever the electrode touches; and whatever the rapidity with which one writes or draws, the strokes made are neatly engraved on the glass*. If the writing or drawing be done slowly, the strokes will be deeply engraved; their breadth will depend on the diameter of the wire serving as electrode; if it is pointed, the strokes can be made extremely fine.

The engraving can be executed with either of the electrodes; but a less-powerful current is required for engraving with the negative.

Although I have obtained these results by using secondary batteries, it is clear that, for continuous work, any other source of electricity can be employed in preference, if the quantity and tension be sufficient—either a Bunsen pile of a sufficient number of elements, or a Gramme machine, or even a magneto-electric machine with currents alternately positive and negative.—*Annales de Chimie et de Physique*, Jan. 1878, tome xiii. pp. 143, 144.

ON THE PHOTOMETRIC COMPARISON OF LIGHT OF DIFFERENT COLOURS. BY PROF. O. N. ROOD, OF COLUMBIA COLLEGE, U.S.A.

The comparison of the intensities of light of different colours has long been considered one of the most difficult of photometric problems; but by the use of very simple means I have recently made a series of measurements of this character which may not be without interest to those whose studies lie in this direction. The luminosity of cardboard painted with vermilion was, for example, measured as follows :—A circular disk of the vermilion cardboard was attached to the axis of a rotation-apparatus, smaller circular

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* There is so often occasion to write or to mark lines on glass in laboratories, that this process will there find frequent applications.

disks of black and white cardboard being simultaneously fastened on the same axis, so that by varying the relative proportions of the latter, a series of greys could be produced at will.

The compound black-and-white disk was now arranged so as to furnish by rotation a grey which was *decidedly darker* than the vermilion; this grey tint was then gradually lightened till the observer became doubtful as to the relative luminosities of the red and grey disks; the angle occupied by the white sector was then measured. Next, a grey decidedly *more* luminous than the vermilion was compared with it, and diminished in brightness till the observer again became doubtful, when a second measurement was taken. All this time the manipulation was performed by an assistant, the experimenter giving directions, but remaining in ignorance of the results to the end. The mean of ten such experiments assigned to the vermilion disk a luminosity of 23·8, that of white cardboard being taken as 100. In this experiment and in all those that follow, proper corrections were made for the amount of white light reflected by the black disk, this having been previously ascertained in a manner which will be described in a future communication.

In order to test the correctness of the final result, the luminosity of a blue-green disk, correctly complementary in colour to the vermilion, was next measured in the same way: it proved to be 26·56. The vermilion and blue-green disks were then combined, according to Maxwell's method, so as to obtain a pure grey by rotation, and the angular proportions of these coloured surfaces and the value of the grey in terms of white and black cardboard measured. The grey thus obtained had a luminosity of 24·54, that of white cardboard being 100. Next, the value of this same grey was *calculated* from the measured luminosities of the two coloured disks, and the proportions of these colours required to produce a pure grey by mixture on the rotation-apparatus; the calculated value was 25·47.

This agreement proves the correctness of the photometric comparison, and also of Grassman's assumption that the total intensity of the mixture of masses of differently coloured light is equal to the sum of the intensities of the separate components, which, so far as I know, has not before received an experimental confirmation.

Corresponding measurements were made with a green and its complementary purple disk; also with a blue and its complementary yellow disk. The results are given below.

	Luminosity.	Grey (observed).	Grey (calculated).
Vermilion	23·8	24·54	25·47
Blue-green	26·56		
Chrome-yellow	80·3	54·51	53·92
Cobalt-blue	35·38		
Green	41·19	24·94	26·26
Purple	14·83		

—Silliman's *American Journal*, February 1878.

XXXVI. *Experiments on the Heat-conductivity of Stone, based on* Fourier's ' *Théorie de la Chaleur.*' *By* W. E. AYRTON *and* JOHN PERRY, *Professors in the Imperial College of Engineering, Tokio, Japan*[*].

[Plates IX. and X.]

I. WHEN a body is a good conductor for heat, it is comparatively easy to find the conductivity correctly by the method employed by Principal Forbes, MM. Despretz, Wiedemann, Franz, and others, which consisted in observing the temperature at different points of a long bar when one end had been kept at a constant temperature sufficiently long for the temperature of any one point of the bar to have become constant. But as the substances for experimenting on became less and less conducting, the amount of heat lost by radiation becomes larger and larger compared with that conducted along the bar ; so that this method of experimenting fails altogether for a non-conducting substance like stone.

In such a case the plan usually adopted has been to measure the amount of heat conducted through a very thin wide sheet of the material when the temperature of each of its surfaces was kept constant. But even when considerable precautions are taken to prevent loss of heat from the edges, &c. (such as those employed by Professor G. Forbes in his experiments published in the Proceedings of the Royal Society of Edinburgh for February 1873), still we feel sure that the results must be

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[*] Communicated by the Authors, having been read before the Asiatic Society of Japan, January 26, 1878.

somewhat doubtful. We are not, therefore, surprised to find
the conductivity of marble to be 0·0048 (gramme, centimetre,
second) as given by M. Péclet in 1841, to be 0·0097 for fine-
grained and 0·0077 for coarse-grained marble, as given by
M. Despretz in 1853, and as 0·0017 as given by Prof. G.
Forbes in 1873.

The method employed by Principal Forbes, and Sir W.
Thomson, in 1860, of deducing the conductivity of rock from
observations of underground temperature is, of course, sus-
ceptible of much greater accuracy than the method referred
to above ; but it has the disadvantage that a considerable period
of time is necessary for the completion of one experiment, and
it can only be performed on a rather large depth of rock form-
ing part of the earth's crust.

The following method which we have employed for deter-
mining the conductivity of heat in stone, and which was
suggested by some remarks made by Sir W. Thomson when
lecturing to the Higher Natural-Philosophy Class, in Glasgow,
in 1874, has the great advantage of perfect certainty in the
results ; and it can be used with comparatively little difficulty
for all bad conductors. The principle consists simply in
keeping a ball of the material to be experimented on in a
water (or other) bath at a constant temperature for a sufficient
length of time for the whole ball to acquire the temperature
of the bath ; then suddenly removing the warm water and
allowing a continuous rapid stream of cold water of constant
temperature to flow round the outside of the ball, while time-
readings of the temperature of some fixed point in the ball (for
example, the centre) are taken as the ball slowly cools. Under
these circumstances one of Fourier's well-known equations
enables us to determine the internal conductivity of the ball,
and the emissivity of the surface.

The obvious difficulty in this method of experimenting is
to determine the temperature, say, at the centre of the ball, at
successive intervals of time, without disturbing the flow of
heat in the sphere. The comparatively small size of our balls
of stone would make this difficulty very considerable if an
ordinary thermometer were used ; but those who have worked
numerical illustrations of Fourier's results will see that the
introduction into the ball of a thermometric junction attached
to very fine leading wires cannot appreciably affect the general
conditions. For absolute correctness it would be necessary
to have a conical tubulure space of which the sides were coated
with a substance impermeable to heat, extending from the
surface of the ball to the centre, and terminating in a small
spherical cavity at the centre, and to employ a thermometric

arrangement of such a nature as would not add to or subtract from the heat at the centre.

One thermoelectric junction being at the centre of the ball, the other may be either kept at a constant temperature, in which case the electric current will be a function of the dif- ference of temperature between the junctions, their mean temperature, the position of the neutral point for the two metals employed, and the slope of their thermoelectric lines ; or the other junction may be immersed in a compensating-bath, which, by being always kept at such a temperature that there is no current, indicates at any moment the temperature of the centre of the ball.

This latter, or *balance*, method was adopted, as it has the following advantages: first, the range of temperature through which the ball falls may be large, and still the galvanometer may be made as sensitive as we please ; second, the balance method may be employed nearly without reference to the " thermoelectric power " of the two metals at different tem- peratures ; and, thirdly, since there is no current, no heat is added to or subtracted from the centre of the ball thermo- electrically, so that no vitiations of the theoretical conditions to ensure accuracy could occur except by heat-conductivity of the thin wires. In reality, of course, as it was impossible to cool the compensating-bath at exactly the same rate as the centre of the ball, there usually was a very weak current ; but as the junction in the bath was as often a very little hotter as a very little colder than that in the ball, the excessively small gains and losses of heat produced by the currents balanced one another.

However, our balance method was employed chiefly for the first two reasons, and not because we feared the abstraction of heat through thermo-electric currents.

II. *Details of the Apparatus.*—In Pl. IX. fig. 1, A is a stone ball 13·8 (thirteen and eight tenths) centims. in diameter, rest- ing on three points in a metal water-bath, B, 17·5 (seventeen and a half) centims. high and 18·3 (eighteen and three tenths) centims. in diameter. This bath had a tap, R, for letting in cold water of constant temperature from a cistern, and a large opening, O, which could be closed by a cork, for suddenly emptying B. The bath stood in a tub, W, to catch the over- flow, as will be described further on. At the centre of the stone ball there was a thermoelectric junction, C, of iron and copper, the wires being carefully insulated from the water and from one another except just at their extremities, where they were bound together and soldered, and immersed in a small drop of mercury to form good thermal contact with the stone.

The cylindrical hole in the stone through which the wires were inserted was only made just large enough to receive them; and the possibility of water entering the hole and making contact with the junction was prevented by the surface of the insulated wires being smeared over with a paste composed of white lead, red lead, and linseed-oil, which by hardening cemented the wires to the stone. The copper wire passed to a key, K, which was connected with one terminal, T, of a delicate deadbeat reflecting-galvanometer of about three quarters of an ohm resistance, which we had constructed for measuring thermal currents. The other end of the iron wire was bound and soldered at J to another copper wire, which was connected with the other terminal, T′, of the galvanometer. D D was a copper compensating water-bath, by means of which the thermoelectric junction J could be always kept at the same, or nearly at the same, temperature as the other junction at the centre of the ball. To ensure the junction J quickly acquiring the temperature of the water in the bath D D, a small perforated plate of copper, P, was soldered to J and hung in the water without touching the sides or bottom of the bath. The bath was divided longitudinally by a perforated copper plate, Q Q, to allow of the water being kept rapidly stirred (to ensure equality of temperature) without risk of the Kew standard thermometer S, which hung in the water, being broken.

This bath was fitted with two taps, U, V—the one for letting in cold water from the cistern, the other for emptying the bath. H is another thermometer hanging in the bath B, and having its bulb surrounded by a small metallic screen to shield it from direct radiation from the ball, but which allowed of free access of the water to the bulb. Either water-bath could be heated by suitable spirit-lamps.

III. *Method of experimenting.*—The two water-baths having been filled with water and left for a sufficiently long time for the temperature of all parts of the apparatus to have become uniform, or very nearly so, a reading of the galvanometer was taken, a small deflection d_1 being obtained. This deflection was due to a small unknown difference of temperature t_1 still remaining between the two junctions. The temperature of J was now raised by a number of small increments, t_2, t_3, &c., producing deflections d_2, d_3, &c. respectively; then, since for small differences of temperature the currents are proportional to the differences, we have, if T is the difference of temperature corresponding with any small deflection D,

$$T = D \frac{\dfrac{t_2}{d_2 - d_1} + \dfrac{t_3 - t_2}{d_3 - d_1} + \dfrac{t_4 + t_3 + t_2}{d_4 - d_1}}{3},$$

the object of taking the mean of a number of observations being, of course, to calibrate the scale near the zero-point with considerable accuracy. This determination of the sensibility of the galvanometer was made before every experiment; and it was usually found that one division of the scale corresponded to rather less than one fiftieth of a degree Centigrade difference of temperature between the junctions when the junctions themselves had a temperature of about 23° C. This amount of delicacy was really more than was absolutely necessary, since the thermometer in the compensating-bath could only be read to the twentieth of a degree.

The baths D D and B were now heated up to about 70° C., and kept at that temperature until the temperature of all parts of the stone ball had become uniform—that is, until there was no current when the two thermometers S and H indicated exactly the same temperature, the small scale-error in H being, of course, allowed for. At this moment the cork at O was removed, the tap R opened, and a quantity of cold water poured into B by means of the tube M M so as to flood the bath B; the whole of the warm water in B was therefore suddenly displaced by cold cistern-water. O was now closed but R left open, so that there was a continual stream of cold water flowing upwards and overflowing the bath at the top. Constant readings of the thermometer in the compensating-bath, combined with simultaneous readings of the galvanometer (the latter being kept as small as possible), were now taken for about 80 minutes, by which time the whole ball had cooled down nearly to the temperature of the cold water. By opening the key K the zero of the galvanometer was frequently taken, to detect any slight change. It was the duty of one observer solely to observe the galvanometer, of another to cool the compensating-bath D D at the right rate and to take readings of the thermometer suspended in it, and of a third to record the time together with the readings made by the other two, as well as to take occasional readings of the thermometer H.

IV. *Reduction of the Readings.*—Fourier's equation for the temperature of a point at a distance x centimetres from the centre of a homogeneous globe, when the globe has been initially all at constant temperature and when it is cooling by a constant external temperature being maintained, is

$$v = v_0 \frac{2 \mathrm{E} r}{\mathrm{K}} \Sigma \frac{\sin\left(\alpha \frac{x}{r}\right)}{\alpha \frac{x}{r}} \frac{\sin \alpha}{\alpha \left(1 - \frac{\sin 2\alpha}{2\alpha}\right)} \epsilon^{-\frac{\alpha^2 \mathrm{K} t}{C r^2}}, \quad . \quad . \quad (1)$$

where v is the excess of temperature of the point over the external temperature on the Centigrade scale;

v_0 is the excess of the initial temperature over the external temperature;

t is the time in seconds since the ball began to cool;

r is the radius of the ball, in centimetres;

K is the internal conductivity, in centimetre-gramme-second units;

E is the surface emissivity, which is such that from a square centimetre of surface 1 Centigrade degree higher than the external medium there will be emitted per second a quantity of heat E;

C is the specific heat of the substance per unit volume;

α is an angle such that

$$1 - \frac{\mathrm{E}r}{\mathrm{K}} = \frac{\alpha}{\tan \alpha}; \quad \cdot \quad \cdot \quad \cdot \quad \cdot \quad \cdot \quad (2)$$

from which equation successive values of α must be found and used to form the different terms under the sign of summation in equation (1). It is evident that the values of α will lie successively in alternate quadrants—the first, third, fifth, &c., or the second, fourth, sixth, &c.,—also that after a certain time has elapsed, depending on the values of E, K, and r, all terms after the first become negligible, so that the true curve of v for a given point becomes a simple logarithmic curve.

If the point is at the centre of the ball, then, as x is nought,

$$\frac{\sin\left(\alpha\,\frac{x}{r}\right)}{\alpha\,\frac{x}{r}} = 1,$$

and the general equation becomes

$$v = v_0\,\frac{2\mathrm{E}r}{\mathrm{K}}\,\Sigma\,\frac{\sin \alpha}{\alpha\left(1 - \dfrac{\sin 2\alpha}{2\alpha}\right)}\,\epsilon^{-\frac{\alpha^2 \mathrm{K}t}{\mathrm{C}r^2}}; \quad \cdot \quad \cdot \quad \cdot \quad (3)$$

or, when all the terms except the first become negligible and therefore α is the smallest positive angle satisfying the equation (2), we have

$$v = v_0\,\frac{2\mathrm{E}r}{\mathrm{K}}\,\frac{\sin \alpha}{\alpha\left(1 - \dfrac{\sin 2\alpha}{2\alpha}\right)}\,\epsilon^{-\frac{\alpha^2 \mathrm{K}t}{\mathrm{C}r^2}}. \quad \cdot \quad \cdot \quad \cdot \quad (4)$$

Equation (4) may also be written in the forms

$$v = 2v_0\,\frac{\sin \alpha - \alpha \cos \alpha}{\alpha - \sin \alpha \cos \alpha}\,\epsilon^{-\frac{\alpha^2 \mathrm{K}t}{\mathrm{C}r^2}} \quad \cdot \quad \cdot \quad \cdot \quad \cdot \quad (5)$$

and

$$v = N\epsilon^{-mt}, \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (6)$$

where N and m are supposed to be constant for the curve.

Now it is easily seen that when K is considerable with regard to r, as in ordinary balls of metal, the period after which all other terms than the first are negligible is exceedingly short, and that this period is longer as the conductivity is less or the radius of the ball is greater; and as we intend to use observations made after this period, there is a certain size of ball for any given material which is most convenient for experiment, so that it is of some importance to get a rough idea of the values of K and E before deciding on the size of the ball. From certain considerations of the possible errors in this method of determining K, it will also be seen, further on, that it may sometimes be necessary to make two sets of experiments on a given material—the first to determine K roughly, the second with accuracy.

Using the observations of which we have spoken, it is evident that $-\dfrac{1}{v}\dfrac{\Delta v}{\Delta t}$, which can easily be measured from the curve plotted from the observations, is m of equation (6), and that N may be determined from any observation, since

$$N = v\epsilon^{mt};$$

so that if a time-curve has been drawn from the observations, m and N may be obtained from a very great number of points in the curve. Also v_0 is known; and

$$\frac{N}{2v_0} = \frac{\sin\alpha - \alpha\cos\alpha}{\alpha - \sin\alpha\cos\alpha}; \quad . \quad . \quad . \quad . \quad (7)$$

so that α can be calculated. To calculate α when we know the value of this expression, a Table of the following kind will be found useful, in which it may be assumed that increase in the expression, for small increments, is proportional to α:—

α, in degrees.	α, in radians.	$\dfrac{\sin\alpha - \alpha\cos\alpha}{\alpha - \sin\alpha\cos\alpha}.$
114·59	1·9998	0·753
115·89	2·0225	0·738
120·00	2·0944	0·757
131·50	2·2953	0·813
147·16	2·5685	0·893

When α is known, we can find K; for

$$m = \frac{K\alpha^2}{Cr^2},$$

or

$$K = \frac{mCr^2}{\alpha^2}. \quad . \quad . \quad . \quad . \quad . \quad (8)$$

Also

$$\frac{Er}{K} = 1 - \frac{\alpha}{\tan \alpha};$$

so that E may be found. After the following illustrations of the method described above, we shall consider the possible errors involved in calculating K and E in this way.

V. As a first example, we shall take the time-curve of fall of temperature of the centre of a stone ball of $5\frac{1}{2}$ centims. radius, which we obtained when the initial temperature of the whole ball was 70° C., and the external temperature kept at 21°·3 C. during cooling. For reasons given further on, the first point we employ in the curve is when t equals 616 seconds from the commencement of cooling. The values of v, m, and $\log N$ corresponding with this and subsequent values of t are given in the following Table:—

t.	v.	m.	$\log N$.
0	48·7		
616	25·72	0·00178	1·93656
691	22·45	184	1·94149
766	19·4	206	1·94215
841	16·62	196	1·94057
916	13·32	209	1·93867
991	12·3	195	1·93649
1066	10·65	188	1·93795
1141	9·2	203	1·93847
1216	7·9	203	1·93643
1291	6·77	207	1·93371

$$\text{Mean } m \quad = 0·00197,$$
$$\text{Mean } \log N = 1·93825,$$
$$\text{Mean N} \quad = 86·746.$$

In this case

$$\frac{\sin \alpha - \alpha \cos \alpha}{\alpha - \sin \alpha \cos \alpha} = \frac{N}{2v_0}$$
$$= \frac{86·746}{97·4}$$
$$= 0·891;$$

an1 we find from the Table given above that

$$\alpha = 146°\cdot76$$
$$= 2\cdot561 \text{ radians.}$$

Therefore K, which equals

$$\frac{mCr^2}{\alpha^2},$$

$$= \frac{0\cdot00197 \times 0\cdot5738 \times (5\cdot5)^2}{(2\cdot561)^2}$$
$$= 0\cdot00520 ;$$

and E, which equals $\dfrac{K}{r} \left(1 - \dfrac{\alpha}{\tan \alpha} \right)$,

$$= 0\cdot00464,$$

the value of α obtained above being, as will be explained near the end of the paper, outside the limits within which small errors in K and E may be expected.

Proceeding in a slightly different way, and determining each value of N from each separate value of m, and using the mean value of N as before, we find

$$K = 0\cdot00518,$$
$$E = 0\cdot00502,$$

the considerable difference between this value of E and that obtained above illustrating what we say at the end of the paper regarding errors.

The above observations were made very early in the inves- tigation, when we were not sufficiently impressed with the importance of keeping the external temperature really con- stant ; and hence the values of $\dfrac{1}{v} \dfrac{dv}{dt}$ differ considerably at dif- ferent parts of the curve. We shall afterwards, however, give our reasons for believing that this does not much alter the value of K, although it seriously affects that of E.

VI. In September 1876 we obtained the curve A A A, Pl. X. fig. 2, for the cooling of the centre of a stone ball 6·9 centims. radius, initially heated to 69°·62 C., the temperature of the stream of water flowing outside being carefully measured at every instant and plotted, giving the curve $a\,a\,a$, fig. 2, the scale for time in both curves being such that O X represents fifty minutes. To obtain K and E from these curves we em- ployed four different methods, as follows :—

First method.—The external temperature having been plotted from the time when the curve for the internal temperature be- came logarithmic, the external-temperature curve was pro- duced until it cut the axis corresponding with time nought,

which it did at a point corresponding with 20°·1 C. This subtracted from 69°·62, the initial internal temperature, was taken to represent v_0; so that

$$v_0 = 49°·52:$$

the values of v were calculated by subtracting from each value of T (the internal temperature shown by the curve A A A) the corresponding value of x (the external temperature shown by the curve $a\,a\,a$). From the various values of t, the time, and of v the corresponding values of m and $\log$ N were calculated; and the series of numbers obtained is shown in the following Table:—

$t.$	T.	$x.$	$v.$	$m.$	$\log$ N.
1050	44·7	19	25·7	0·00110	1·90014
1200	40·37	18·69	21·68	113	1·80629
1350	36·76	18·38	18·38	110	1·89462
1500	33·68	18·08	15·60	109	1·89341
1650	30·98	17·76	13·22	110	1·89155
1800	28·75	17·46	11·29	105	1·89304
1950	26·8	17·15	9·65	104	1·89491
2100	25·21	16·9	8·31	099	1·90002
2250	23·88	16·74	7·14	101	1·90415
2400	22·73	16·6	6·13	101	1·90793
2550	21·7	16·5	5·2	109	1·90650
2700	20·75	16·4	4·35	119	1·89902

$$\text{Mean } m \ = 0·001075$$
$$\text{Mean } \log \text{N} = 1·89846$$
$$\text{Mean N} \ = 79·15.$$

Hence

$$\frac{\sin \alpha - \alpha \cos \alpha}{\alpha - \sin \alpha \cos \alpha} = \frac{\text{N}}{2v_0}$$
$$= \frac{79·15}{99·04}$$
$$= 0·7999;$$

consequently

$$\alpha = 128°·62,$$
$$\text{or } 2·2445 \text{ radians};$$

whence

$$\text{K} = 0·00583,$$

and

$$\text{E} = 0·00236.$$

Second method.—As it is still an unsolved problem to find the general solution for loss of temperature in a globe when the external conditions are varying, the form of the functions being different from those employed in Fourier's solution, we

were led to make the assumption that if T° C. is the temperature of the centre and x° C. is the outside temperature at any instant, and if T_0° C. was the initial temperature of the centre, then Fourier's equation becomes

$$T - x = 2(T_0 - x)\frac{\sin \alpha - \alpha \cos \alpha}{\alpha - \sin \alpha \cos \alpha} \epsilon^{-\frac{\alpha^2 K t}{C r^2}}, \quad . \quad . \quad . \quad (9)$$

or

$$\log (T - x) = \log 2 + \log (T_0 - x) + \log \frac{\sin \alpha - \alpha \cos \alpha}{\alpha - \sin \alpha \cos \alpha} - \frac{\alpha^2 K t}{C r^2}.$$

Differentiating with respect to t, we have

$$\frac{1}{T - x}\frac{d}{dt}(T - x) = \frac{1}{T_0 - x}\frac{dx}{dt} - \frac{\alpha^2 K}{C r^2},$$

from which $\dfrac{\alpha^2 K}{C r^2}$, or m, can be found ; and when this is determined we can find α, since we can find from equation (9) the value of

$$\frac{\sin \alpha - \alpha \cos \alpha}{\alpha - \sin \alpha \cos \alpha},$$

which is called R in the following list of numbers, obtained from the curves A A A and $a\,a\,a$.

t.	T.	x.	m.	R.	α°.	K.	E.
0	60·62						
1050	44·70	19·00	0·00114	0·840	136·90	0·00546	0·00281
1200	40·37	18·69	115	0·846	131·1	541	289
1350	36·76	18·38	114	0·835	135·90	553	277
1500	33·68	18·08	113	0·824	133·7	567	265
1650	30·98	17·76	111	0·795	129·98	608	242
1800	28·75	17·46	108	0·756	119·79	675	220
1950	26·80	17·15	106	0·726	113·33	741	199
2100	25·21	16·94	104	0·700	107·87	793	187
2250	23·88	16·74	105	0·717	111·44	758	194
2400	22·73	16·60	108	0·772	123·15	639	223
2550	21·70	16·50	115	0·919	152·36	443	249

Mean K = 0·00624.

Mean E = 0·00237.

Third method.—Assuming that the curve from 1050 seconds is logarithmic, and satisfies the equation

$$v = N\epsilon^{-mt},$$

where N and m are constants, and that the outside temperature x° C. is constant but *unknown*, we can find by the method of least squares the value of x° and of $-\dfrac{1}{v}\dfrac{dv}{dt}$, or m, in the

above equation. Choosing, for convenience, Δt equal to 150 seconds, and putting

$$-\frac{\Delta v}{v} = \frac{1}{y},$$

we find from the curve A A A :—

t.	T.	Equations.
1125	42·53	$4\cdot33\,y + x = 42\cdot53$
1275	38·56	$3\cdot61\,y + x = 38\cdot56$
1425	35·22	$3\cdot08\,y + x = 35\cdot22$
1575	32·33	$2\cdot70\,y + x = 32\cdot33$
1725	29·86	$2\cdot23\,y + x = 29\cdot86$
1875	27·77	$1\cdot95\,y + x = 27\cdot77$
2025	26·00	$1\cdot59\,y + x = 26\cdot00$
2175	24·54	$1\cdot33\,y + x = 24\cdot54$
2325	23·30	$1\cdot15\,y + x = 23\cdot30$
2475	22·21	$1\cdot03\,y + x = 22\cdot21$
2625	21·22	$0\cdot95\,y + x = 21\cdot22$

Adding the above equations, we get

$$23\ 95\,y + 11x = 323\cdot54.$$

Multiplying each equation by the coefficient of y in it and adding, we find

$$64\cdot91\,y + 23\cdot95x = 783\cdot8\,;$$

and from these two equations we find

$$x = 15°\cdot87\ \text{C.,}$$

$$\frac{1}{y} = 0\cdot1609,$$

and therefore

$$m \text{ or } \frac{1}{v}\frac{dv}{dt} = 0\cdot00107.$$

These values of x and m were applied to every observation to determine N; and the values of log N so obtained are given below :—

$$\log \text{N.}$$
$$1\cdot9336$$
$$1\cdot9324$$
$$1\cdot9312$$
$$1\cdot9278$$
$$1\cdot9266$$
$$1\cdot9233$$
$$1\cdot9231$$
$$1\cdot9244$$
$$1\cdot9252$$
$$1\cdot9226$$
$$1\cdot9134$$

From these the mean value of N was found to be 84·29 ; and since

$$\frac{\sin \alpha - \alpha \cos \alpha}{\alpha - \sin \alpha \cos \alpha} = \frac{N}{2v_0}$$

$$= 0·784,$$

it follows that

$$\alpha = 125°·54;$$

so that

$$K = 0·00609,$$

and

$$E = 0·00226.$$

Fourth method.—We now come to the last and most impor-tant method, to use which, however, requires a certain amount of experience in curves of this kind. If the stream of water outside the ball had been kept exactly at the lowest tempera-ture (16°·4 C.) all the time, then the centre would have cooled more quickly at the beginning, and the last observation would have been less than it is. A curve for the cooling of the centre when the external temperature is kept perfectly constant we shall call, for brevity, an *exisothermal* curve. We see there-fore that the exisothermal curve for 16°·4 C. will be altogether below the curve A A A, and the exisothermal curve for 19°·9 will be below A A A for some distance at the beginning, and that it then cuts A A A and remains above it.

If there are two exisothermals for the external temperatures x_1 and x_2 with the same initial temperature of the centre T, then, since at any moment $\dfrac{v}{v_0}$ of the one equals $\dfrac{v}{v_0}$ of the other, it follows, if P″A and P″B (fig. 3) are the exisothermals in ques-tion, and if OA″ represents x_1 and OB″ represents x_2, that

$$\frac{A'P}{A''P''} = \frac{B'Q}{B''P''} ;$$

so that if the point P is given and we want to find PQ, we have the equation

$$PQ = (x_2 - x_1)\left(1 - \frac{A'P}{T - x_1}\right).$$

Now it will be observed from this formula that all the exiso-thermal curves that we can draw from 19°·9 to 16°·4 C. almost coincide with one another for small values of the time, and even at 2700 seconds their distance asunder is not very great.

With practice it is not difficult to see between what exiso-thermal curves lie the different parts of the curve obtained

from the experiments. For example, the curve A A A lies between the following exisothermals:—

when $t = 1200$, between the exisothermals for $\overset{\circ}{1}9\cdot3$ and $\overset{\circ}{1}8\cdot69$

,,	1500	,,	,,	,,	18·69 ,,	18·08
,,	2100	,,	,,	,,	17·76 ,,	17·15
,,	2400	,,	,,	,,	16·94 ,,	16·60
,,	2700	,,	,,	,,	16·6 ,,	16·4.

If this is true, then the ordinates of the curve A A A are greater than the ordinates of the exisothermal for 16°·4 C. by distances which are approximately slightly less than

$$2\cdot9\left(1 - \frac{23\cdot97}{50\cdot93}\right) \text{ or } 1\cdot53 \text{ (say therefore equal to } 1\cdot50 \text{ for } t = 1200),$$

and slightly less than

$$0\cdot2\left(-1\frac{4\cdot35}{53\cdot22}\right) \text{ or } 0\cdot184 \text{ (say therefore equal to } 0\cdot18 \text{ for } t = 2700).$$

Consequently, as in an exisothermal curve,

$$m = \frac{\log\frac{v_1}{v_2}}{(t_2 - t_1)\log\epsilon},$$

it follows that in our experiment m is very nearly equal to

$$\frac{\log\frac{40\cdot37 - 16\cdot4 - 1\cdot50}{20\cdot75 - 16\cdot4 - 0\cdot18}}{1500\log\epsilon} \text{ or } 0\cdot00112,$$

and

$$N = 417\,\epsilon^{2700m}$$
$$= 85\cdot79$$
$$\frac{N}{2v_0} = 0\cdot808;$$

from which in the usual way it may be found that

$$\alpha = 130°\cdot47,$$
$$K = 0\cdot00590,$$
$$E = 0\cdot00252.$$

We see, therefore, that by these four methods of treating the curves A A A, $a\,a\,a$ (fig. 2), which have been rendered necessary by the want of constancy of the external temperature, we have obtained the following results for K and E:—

	1st.	2nd.	3rd.	4th.
K	0·00583	0·00624	0·00609	0·00590
E	0·00236	0·00237	0·00226	0·00252

Now, as the present investigation was designed to show the feasibility of the experiments, and the possibility of deducing from them the numerical values of K and E, rather than to obtain with the greatest possible accuracy the values of K and E for one particular Japanese stone, we have not thought it necessary before the publication of this paper to repeat the experiments, using greater precautions to ensure perfect uniformity of the outside temperature by making the water-bath larger and the stream of cold water more rapid. And although, had we done so, any one of the above four methods would have given results differing extremely little from the truth, still it would have been none the less interesting to have examined, as we have done, what method of reduction is calculated to give the most accurate results when there is a slight variation in the external temperature during the course of an experiment.

Where considerable accuracy is required we should advise the employment of the fourth method; if still greater accuracy is desired, then, several sets of observations having been made, the values of K and E should first be calculated by the fourth method; then, the mean of all the values of K having been taken as well as the mean of all the values of E, these results should be employed in drawing a number of exisothermal curves, when, lastly, the way in which the curves obtained from experiment lie among these exisothermals will enable us to calculate, again by the fourth method, the final values of K and E with any desired amount of accuracy. To illustrate this, we have drawn a number of exisothermals on the supposition that

$$K = 0·00590,$$
$$E = 0·00252;$$

but the size of fig. 2, as engraved, only enables us to show two, B B B, C C C, the exisothermals for 16°·4 C. and 19°·3 C.

From an examination of the way in which our curve A A A, obtained from the experiments, lies among these exisothermals, it will be seen that our first approximation is quite accurate enough for practical purposes, so that further approximations are unnecessary in this case unless very great accuracy be desired.

VII. We made another series of experiments at the end

of September 1876, on the same stone ball of 6·9 (six and nine tenths) centims. radius, and found that the fourth method, applied to the curve of falling temperature between 1050 seconds and 2100 seconds, gave

$$K = 0·00578,$$
$$E = 0·00263.$$

We did not use the earlier part of the curve, as we were desirous that all terms except the first should disappear in the calculations. The portion after 2100 seconds we also did not employ, as it is more difficult then to judge between what isothermals lies the curve obtained from the experiments.

In the early part of September, 1876, we obtained the curve D D D, fig. 4, for the cooling of the centre of a stone ball of 5·5 (five and a half) centims. radius, initially heated to 73°·62 C., represented by O A, the temperature of the stream of water outside when plotted giving the curve $d\,d\,d$, the scale of time in both curves being such that O X represents 50 minutes. From these two curves, using the fourth method, we found that

$$K = 0·00548,$$
$$E = 0·00495 ;$$

on the assumption that the curve is within the exisothermal for 21°·5 C. at 450 seconds, and within the exisothermal for 20°·2 C. at 1200 seconds. In the above calculations, although there is reason for believing that the stone, while of the same general character, differs somewhat in the two balls, we were compelled to use in both cases the same value of the specific heat (0·5738) per unit volume obtained experimentally from some fragments of the stone remaining after the turning. Our results obtained by the fourth method are therefore:—

Radius of ball, in centimetres.	Initial temperature, in degrees Centigrade.	Outside temperature at 2700 seconds.	K.	E.
6·9	69·62	16·4	0·00590	0·00252
6·9	69·25	17·75	0 00573	0·00263
5·5	73·62	17·9	0·00548	0·00495

As we have only drawn attention to four curves obtained from the experiments, one in § V. and three in § VII., it may be as well to mention that several other curves were drawn from sets of observations made during July, August, and September in the earlier part of the investigation. As, however,

the special object of these earlier experiments was to familiarize us with the method, we have not employed the results so obtained for the calculation of the conductivity.

The considerable difference between the value of E obtained for the ball of $5\frac{1}{2}$ centims. radius and for that of $6\frac{9}{10}$ centims. radius is due, to a certain extent, to the difference in the stone and difference in the surface, the smaller ball being of finer grain and having a smoother surface. The rock from which the balls were turned is a fine-grained variety of stone, which is largely used for building-purposes in many parts of Japan. Our colleague, Mr. J. Milne, Professor of Geology at this College, informs us that, with its varieties, it is a typical rock of the country, forming in many districts large mountain-ranges. It is essentially felspathic; and the particular specimens employed were, in the original state, probably a porphyritic trachyte, and, from the traces of hornblendic crystals which are apparently to be found in it, was also rhyolitic. Both the felspathic base and the enclosed crystals have been decomposed, especially the latter, which have been altogether kaolinized. Although the rock is generally light-coloured, it carries with it a slight greenish tinge, due to the chloritization of a portion of the hornblende. The crystals of hornblende and the felspar are all of small dimensions, probably never more than two or three millimetres long; so that the specimens have as a mass a fine-grained homogeneous character. They also do not appear to be at all calcareous. Owing to the nature of the products of decomposition, the rock is soft and probably porous, and therefore to a certain extent permeable to water.

VIII. We now pass to the consideration of the possible errors made in the determination of K and E by the method of experimenting that we have adopted. In the actual thermometer-readings there need not have been an error of more than $0°\cdot05$ C.; but a single measurement of the actual temperature of the centre of the stone ball may possibly have erred by as much as $0°\cdot2$ C., due to want of perfect equalization of temperature in the compensating-bath &c. As, however, the observations were all corrected by drawing time-curves, such errors were eliminated to an indefinite extent: and here it may be stated that regular curves were found to pass through nearly every point, as plotted from the observations on a large sheet of squared paper.

Our methods of reduction being based on Fourier's mathematical calculations, really consist in finding exisothermal curves from the given observations. Suppose such an exisothermal curve found, then the value of v for any time t will be subject to an error which we may call δv. Now it may be

easily shown that practically

$$\frac{\delta m}{m} = \frac{\delta v}{v};$$

and we know from experience that there is no great error in the determination of m unless the determination is made from very small values of v: consequently $\dfrac{\delta v}{v}$ is small. Again, since

$$N = v\epsilon^{mt}$$

$$\frac{\delta N}{N} = mt\frac{\delta m}{m} + \frac{\delta v}{v};$$

and as m is about 0·001 in our experiments, mt will be practically between 1 and 3 for values of t between 1000 and 3000 seconds, so that

$$\frac{\delta N}{N} \text{ is between 2 and 4 times } \frac{\delta v}{v}$$

If we suppose the errors in the measurements of r and C (the radius and specific heat per unit volume) to be negligible, we have for the error δK in K, arising from an error δv in v and δv_0 in v_0,

$$\frac{\delta K}{K} = \frac{\delta m}{m} - 2\frac{\delta\alpha}{\alpha},$$

$$= \frac{\delta v}{v} - 2\frac{\delta\alpha}{\alpha} \text{ approximately.}$$

But it may be easily shown that the error $\delta\alpha$ in α is such that

$$\frac{\delta\alpha}{\alpha} = \left(\frac{\delta N}{N} - \frac{\delta v_0}{v_0}\right)\frac{N}{2v_0}\frac{(\alpha - \sin\alpha\cos\alpha)^2}{\alpha\sin\alpha(\alpha^2 + \alpha\sin\alpha\cos\alpha - 2\sin^2\alpha)}$$

$$= \left(2 \text{ to } 4\frac{\delta v}{v} - \frac{\delta v_0}{v_0}\right)\frac{(\sin\alpha - \alpha\cos\alpha)(\alpha - \sin\alpha\cos\alpha)}{\alpha\sin\alpha(\alpha^2 + \alpha\sin\alpha\cos\alpha - 2\sin^2\alpha)}; \quad (10)$$

consequently it follows that the value of $\dfrac{\delta K}{K}$ will, as a rule, really depend on the value of the trigonometrical expression in equation (10).

Similarly we may prove that

$$\frac{\delta E}{E} = \frac{\delta m}{m} + \frac{\delta\alpha}{\alpha}\frac{\alpha^2 + \alpha\sin\alpha\cos\alpha - 2\sin^2\alpha}{\sin\alpha(\sin\alpha - \alpha\cos\alpha)}$$

$$= \frac{\delta v}{v} + \left(2 \text{ to } 4\frac{\delta v}{v} - \frac{\delta v_0}{v_0}\right)\frac{\alpha - \sin\alpha\cos\alpha}{\alpha\sin^2\alpha} \text{ approximately; } (11)$$

therefore the value of δE will, as a rule, depend on the value of the trigonometrical expression in the last equation. Consequently we have calculated, as follows, the values of the trigonometrical expressions in equations (10) and (11) for different values of α :—

α, in degrees.	$\dfrac{(\sin \alpha - \alpha \cos \alpha)(\alpha - \sin \alpha \cos \alpha)}{\alpha \sin \alpha \, (\alpha^2 + \alpha \sin \alpha \cos \alpha - 2 \sin^2 \alpha)}$.	$\dfrac{\alpha - \sin \alpha \cos \alpha}{\alpha \sin^2 \alpha}$.
20	64·965	0·67789
30	18·304	0·69205
50	6·6181	0·74277
70	3·5864	0·83524
90	2·1975	1·0023
110	1·5223	1·3220
130	1·2369	2·0740
140	1·1927	2·9081
150	1·2356	4·6616
160	1·4506	9·5326
170	2·7473	35·073
180	∞	∞

The curve A B, fig. 5, shows the scale of possible error in K for different values of α; and the curve C D shows the scale of possible error in E. We see, therefore, that for given errors in v and v_0 the error in K is very great for small values of α; so that, for instance, values of α less than $50°$ are very unsuitable for use in calculations to determine K; and therefore, if we had used such a size of ball as gave a value of α less than $50°$, we should have had to employ a new ball of more suitable size to get a more correct answer. There is a diminution in the possible error of K as α increases to about $140°$; and from $160°$ the possible error increases rapidly and becomes infinite when $\alpha = 180°$. The error in E (the emissivity) is least for small values of α, and it increases with α. Thus, to find K and E with errors of approximately the same amount, we ought to use such a size of ball as will make α about $120°$. If K only is wanted accurately, we ought to make α about $140°$; but if E only is wanted, then α should be made as small as possible.

We here append a list of accurately determined values of α for different values of $\dfrac{Er}{K}$, which Table, for reference further on, has been continued beyond α equals $180°$:—

S 2

α, in degrees.	α, in radians.	$\dfrac{\alpha}{\tan \alpha}$.	$\dfrac{\mathrm{E}r}{\mathrm{K}}$.	$\log \dfrac{\sin \alpha}{\alpha - \sin \alpha \cos \alpha}$.
$1\overset{\circ}{0}$	0·174533	0·989826	0·010174	1̄·6927641
20	0·349066	0·959051	0·040949	1̄·0920079
30	0·523599	0·906900	0·093100	0·7419074
40	0·698132	0·832001	0·167999	0·4947749
50	0·872665	0·732253	0·267747	0·3041719
60	1·047198	0·604600	0·395400	0·1492315
70	1·221731	0·444674	0·555326	0·0185805
80	1·396263	0·246199	0·753801	1̄·9051258
90	1·570796	0	1·000000	1̄·8048803
100	1·745329	− 0·307749	1·307749	1̄·7108789
110	1·919862	− 0·698773	1·698773	1̄·6224941
120	2·094395	− 1·209200	2·209200	1̄·5348553
130	2·268928	− 1·903857	2·903857	1̄·4431355
140	2·443461	− 2·912004	3·912004	1̄·3403313
150	2·617994	− 4·534490	5·534490	1̄·2145267
160	2·792527	− 7·672423	8·672433	1̄·0407430
170	2·967060	− 16·827030	17·827030	2̄·7430077
180	3·141593	∞	....	− ∞
190	3·316126	18·806681	....	2̄·7420334
200	3·490659	9·590509	....	1̄·0330932
210	3·665191	6·348295	....	1̄·1894748
220	3·839724	4·576005	....	1̄·2833701
230	4·014257	3·368360	....	1̄·3374829
240	4·188790	2·418400	....	1̄·3628306
250	4·363323	1·588120	....	1̄·3663971
260	4·537856	0·800147	0·199853	1̄·3531834
270	4·712389	0	1·000000	1̄·3267590
280	4·886922	− 0·861696	1·861696	1̄·2893785
290	5·061455	− 1·842220	2·842220	1̄·2419737
300	5·235988	− 3·023000	4·023000	1̄·1840241
310	5·410521	− 4·539861	5·539861	1̄·1131865
320	5·585054	− 6·656008	7·656008	1̄·0243454
330	5·759587	− 9·975900	10·975900	2̄·9070970
340	5·934120	− 16·303865	17·303865	2̄·7377888
350	6·108652	− 34·643888	35·643888	2̄·4417337
360	6·283185	∞	....	− ∞
370	6·457718	36·623539	....	2̄·4412471

Table (*continued*).

α, in degrees.	α, in radians.	$\dfrac{\alpha}{\tan \alpha}$.	$\dfrac{\mathrm{E}r}{\mathrm{K}}$.	$\log \dfrac{\sin \alpha}{\alpha - \sin \alpha \cos \alpha}$.
380°	6·632251	18·221958	....	$\bar{2}$·7339632
390	6·806784	11·789695	....	$\bar{2}$·8945736
400	6·981317	8·320010	....	$\bar{2}$·9958956
410	7·155850	6·004471	....	$\bar{1}$·0605549
420	7·330383	4·232199	....	$\bar{1}$·0988471
430	7·504916	2·731566	....	$\bar{1}$·1166483
440	7·679449	1·354095	....	$\bar{1}$·1178020
450	7·853982	0	1·000000	$\bar{1}$·1049101
460	8·028515	− 1·415644	2·415644	$\bar{1}$·0795630
470	8·203047	− 2·985665	3·985665	$\bar{1}$·0423199
480	8·377580	− 4·836798	5·836798	$\bar{2}$·9925255
490	8·552113	− 7·176075	8·176075	$\bar{2}$·9278685
500	8·726646	−10·400014	11·400014	$\bar{2}$·8433814
510	8·901179	−15·417294	16·417294	$\bar{2}$·7288930
520	9·075712	−24·935314	25·935314	$\bar{2}$·5620578
530	9·250245	−52·460735	53·460735	$\bar{2}$·2655615
540	9·424778	∞	....	$-\infty$

In determining the most suitable size for our ball there are some other points to be considered:—

First, for a given size of ball let us find the period after which the time-curve for v becomes a simple logarithmic curve —that is, the period after which

$$\frac{\sin \alpha_1}{\alpha_1 \sin \alpha_1 \cos \alpha_1} \, \epsilon^{-\frac{\alpha_1^2 \mathrm{K} t}{\mathrm{C} r^2}}$$

is many times greater than

$$\frac{\sin \alpha_2}{\alpha_2 - \sin \alpha_2 \cos \alpha_2} \, \epsilon^{-\frac{\alpha_2^2 \mathrm{K} t}{\mathrm{C} r^2}}.$$

Now for values of α such as we have been considering, the first trigonometrical coefficient is found to be two or more times the second, so that the second term will be negligible for a value of t such that,

$$\epsilon^{-\frac{\alpha_1^2 \mathrm{K} t}{\mathrm{C} r^2}} > 100 \, \epsilon^{-\frac{\alpha_2 \mathrm{K} t}{\mathrm{C} r^2}},$$

or

$$-\frac{\alpha_1^2 \mathrm{K} t}{\mathrm{C} r^2} \log_{10} \epsilon > 2 - \frac{\alpha_2^2 \mathrm{K} t}{\mathrm{C} r^2} \log_{10} \epsilon,$$

or

$$\alpha_2^2 - \alpha_1^2 > \frac{2Cr}{Kt \log_{10} \epsilon}.$$

It will be found that

$$\alpha_2^2 - \alpha_1^2 = 20 \text{ approximately in all cases;}$$

so that the above consideration reduces itself to

$$Cr^2 < 10Kt \log_{10} \epsilon,$$

or

$$\frac{Cr^2}{K} < 4{\cdot}343\,t. \quad . \quad . \quad . \quad . \quad . \quad . \quad (12)$$

For small values of α, such as we have not to consider in calculations of K, we use a different number from 2 in the ratio of inequality and we get different results. Notably we see that when α is very small, as in Mr. McFarlane's experiments (described in Proc. Roy. Roc. 1873), and in those on radiation in different gases at different pressures which we are at present carrying on, the second term in the series almost at once becomes insignificant.

Second. Our next consideration, in connexion with the determination of the size of the ball, is to arrange that the fall of temperature shall not be too rapid; otherwise it cannot be accurately measured, nor can the external temperature be kept constant. We may take it for granted that when v is, say, 50° C., the fall of temperature should not exceed 1° C. in 15 seconds, or

$$-\frac{dv}{dt}, \text{ or } mv, \, = \frac{1}{<15};$$

so that m, or $\dfrac{\alpha^2 K}{Cr^2}$, must be equal to or less than

$$\frac{1}{15 \times 50}, \text{ or } 0{\cdot}00133.$$

This condition, for different values of α, becomes as follows:—

If $\alpha = 10$, then $\dfrac{Cr^2}{K}$ must be equal to, or greater than, 22·904

= 40,	,,	,,	,,	,,	366·54
= 80,	,,	,,	,,	,,	1465·9
=110,	,,	,,	,,	,,	2771·3
=120,	,,	,,	,,	,,	3298·1
=130,	·,	,,	,,	,,	3870·9
=140,	,,	,,	,,	,,	4489·2

(13)

Now we have seen from the curves A B, C D (fig. 5), that the conditions of equal and small errors in K and E necessitates α being about 120°; consequently conditions (13) require that $\dfrac{Cr^2}{K}$ must be equal to, or greater than, 3298·1. But equation (12) requires that $\dfrac{Cr^2}{K}$ should be less than $4 \cdot 343\,t$;

$$\therefore\ 4 \cdot 343\,t \text{ must be } > 3298 \cdot 1,$$

or

$$t \text{ must be } >\ 759 \text{ seconds};\quad .\quad .\quad (14)$$

and this result is independent of the values of C, r, K, and E. If, however, it be desired to obtain the minimum possible error in K only, then the curve A B (fig. 5) shows us that α should be about 140°; therefore conditions (13) require that

$$\dfrac{Cr^2}{K} \text{ should be equal to, or greater than, } 4489 \cdot 2.$$

Combining this with equation (12), we see that

$$4 \cdot 343\,t \text{ must be } > 4489 \cdot 4,$$

or

$$t \text{ must be } > 1034 \text{ seconds,}$$

a result also independent of the values of C, r, K, and E. Condition (14) is independent of the values of C, r, K, and E; but substituting in equation (12) known values of C and K, we obtain a higher inferior limit for t. In our experiments, for example,

$$C = 0 \cdot 5738,$$

and

$$K = 0 \cdot 006 \text{ about};$$

so that for a ball of 6·9 centims. radius, equation (12) leads to the result that

$$t \text{ must be } > 1072 \text{ seconds.}$$

If, again, we only wish that the possible error in E shall be a minimum, then α should be small; consequently $\dfrac{Cr^2}{K}$ may be small, and therefore t; that is, we may use the curve from the beginning.

We might here return to our Table of the values of $\dfrac{Er}{K}$; and remembering that α must be about equal to 120 to give small and equal possible errors in K and E, we could determine the size of the ball of given conductivity and emissivity most suitable for experiment. Thus we might show that, even

with the largest value of E which would allow us with run-
ning water to keep the outside temperature approximately
constant, the radius of an experimental ball to determine the
conductivity of copper would need to be greater than 500 cen-
tims., and hence impracticable ; but if a more effective method
could be employed of keeping the outside temperature con-
stant, then, making E, say, 0·01, the radius of the copper ball,
to give satisfactory results, ought to be between 50 and 400
centims.   It is quite evident, from such considerations, that
our method of experimenting can only be used with substances
having a heat-conductivity between 0·0003 and 0·03.

At the commencement of our investigation we chose rather
vaguely balls of 5·5 and 6·9 centims. radius, being guided only
by former experience in numerical calculations under Sir W.
Thomson, not having at the beginning of our experiments
worked out Fourier's formula, and being quite unable to obtain
either Fourier's treatise *De la Chaleur*, or any mathematical
assistance in Japan.

Now we see that in order that α should equal 120°,

$$\frac{Er}{K} \text{ should equal } 2·21 ;$$

that is, for the smaller ball,

$$\frac{E}{K} \text{ should equal } 0·4 ;$$

and for the larger,

$$\frac{E}{K} \text{ should equal } 0·32 :$$

and we had reason to believe, as our investigation has since
proved to be correct, that $\frac{E}{K}$ had about these values.   If, how-
ever, we had found that $\frac{E}{K}$ was very different from 0·4, then
we should have been compelled either to have made new stone
balls, or to have varied E either by coating the surface of the
ball, or by using, instead of the flowing water, the method of
radiating to an enclosure.

IX. As this is perhaps the first time that any series of ob-
servations have been made illustrating Fourier's mathematical
results, we think that students will be glad to get the obser-
vations as they were obtained, without any correction whatever,
from which the curves A A A, *a a a* (fig. 2) were drawn.
They are as follows:—

September 25, 1876.—*Experiments on the stone ball of* 6·9
centims. radius.

The galvanometer-scale having been calibrated, it was found
that a deflection of one division on the scale to the right from
the zero indicated that the junction at the centre of the ball
was cooler than that in the compensating-bath by 0·022 C.
when the temperatures of the junctions was about 23° C. The
ball was then kept at 70° C. until the whole was at the same
temperature. The hot water was then suddenly removed and
a rapid stream of cold water at a temperature of 17° C. allowed
to flow over the ball, as described in § III.

Time, in minutes.	Observed temperature of compensating bath.	Galvanometer reading.	Galvanometer zero.	Temperature of stream of water.	Value of galvanometer deflection in degrees Centigrade.	True temperature of centre of ball.
0	69·8 C.	5 R	0	68 C.	−0·1 C.	69·7 C.
2	70	32 R	0	23	−0·7	69·3
2½	69·2	2 L	0	......	+0	69·2
3	68·7	8 L	0	......	+0·2	68·9
3½	68·6	9 L	0	......	+0·2	68·8
4	68·7	2 L	0	21	0	68·7
4½	68·6	3 R	0	......	−0·1	68·5
5	68·4	2 R	2 L	......	0	68 4
5½	68·5	14 R	2 L	20·8	−0·3	68·3
6	68·1	21 R	2 L	......	−0·5	67·6
6½	67·2	6 R	0	......	−0·1	67·1
7½	66	13 R	0	20·8	−0·3	65·7
8	66·2	48 R	0	......	−0·1	65·2
8½	66	75 R	0	......	−1·6	64·4
9½	58·1	160 L	0	20·5	+3·5	61·6
10	58·6	95 L	0	......	+2·0	60·6
10½	58·8	43 L	0	......	+1·0	59·8
11	59·2	20 R	1 L	......	−0·5	58·7
11½	59·3	70 R	0	......	−1·5	57·8
12	58	70 R	0	......	−1·5	56·5
12½	56	32 R	0	20·3	−0·7	55·3
13	55·2	50 R	0	......	−1·1	54·1
13½	53·2	22 R	0	......	− 0·5	52·7
14	51·8	10 R	0	20·3	−0·2	51·6
14½	51·4	29 R	0	......	−0·6	50·8
15	49 8	5 R	0	......	−0·1	49·7
15½	49·4	34 R	0	20·3	−0·5	48·9
16	48	15 R	0	......	−0·3	47·7
16½	47·1	20 R	0	......	−0·4	46·7
17	45·7	5 R	0	20·3	−0·1	45·6
17½	44	29 L	0	......	+0·6	44·6
18	43·9	0	0	... ..	0	43·9
18½	43·3	15 R	0	20·3	−0·3	43
19	41·6	21 L	0	......	+0·5	42·1
19½	41·1	5 L	0	20·3	+0·1	41·2
20	40·2	5 L	0	......	+0·1	40·3
21	39·6	33 R	0	......	− 0·7	38·9

Table (*continued*).

Time, in minutes.	Observed temperature of compensating bath.	Galvanometer-reading.	Galvanometer zero.	Temperature of stream of water.	Value of galvanometer deflection in degrees Centigrade.	True temperature of centre of ball.
$21\frac{1}{2}$	38.5 C.	17 R	0	......	-0.4	38.1 C.
22	38	23 R	0	20	-0.5	37.5
$22\frac{1}{2}$	36.8	6 R	0	......	-0.1	36.7
23	36.3	10 R	0	18.5	-0.2	36.1
$23\frac{1}{2}$	35.6	5 R	0	......	-0.1	35.5
24	34.2	27 L	0	18	$+0.6$	34.8
$24\frac{1}{2}$	34.1	8 L	0	......	$+0.2$	34.3
25	33	28 L	0	18	$+0.6$	33.6
$25\frac{1}{2}$	32.9	10 L	0	......	$+0.2$	33.1
26	32.5	6 L	0	......	$+0.1$	32.6
$26\frac{1}{2}$	32	4 L	0	17.9	$+0.1$	32.1
27	31.4	15 L	0	......	$+0.3$	31.7
$27\frac{1}{2}$	31.1	5 L	0	......	$+0.1$	31.2
28	30.8	8 R	0	17.8	-0.2	30.6
29	29.2	29 L	0	......	$+0.6$	29.8
$29\frac{1}{2}$	29.2	9 L	0	......	$+0.2$	29.4
30	28.6	15 L	0	......	$+0.3$	28.9
$30\frac{1}{2}$	28.5	1 L	0	17.8	0	28.5
$31\frac{1}{2}$	27.8	2 L	0	......	0	27.8
32	27.1	11 L	0	......	$+0.2$	27.3
$32\frac{1}{2}$	26.8	5 R	0	17.8	-0.1	26.7
$33\frac{1}{2}$	26.1	6 R	0	......	-0.1	26
35	24.7	15 L	0	17.7	$+0.3$	25
36	24.3	11 L	0	......	$+0.2$	24.5
37	23.7	14 L	0	17.1	$+0.3$	24
38	23	25 L	0	......	$+0.5$	23.5
39	23	6 L	0	17	$+0.1$	23.1
41	22.1	16 L	0	......	$+0.3$	22.4
42	21.7	22 L	0	......	$+0.5$	22.2
43	21.4	12 L	0	17	$+0.3$	21.7
$44\frac{1}{2}$	20.9	13 L	0	......	$+0.3$	21.2
46	20.3	11 L	0	......	$+0.2$	20.5
48	20	75 R	0	17	-1.6	18.4
$50\frac{1}{2}$	17.6	11 L	0	......	$+0.2$	17.8

To illustrate the mathematical results in connexion with the cooling of a globe, given by Fourier in his treatise *De la Chaleur*, we have drawn figures 6 and 7. A ball of 6·9 centims. radius, having an internal conductivity 0·00590 (centim., second) and an emissivity 0·00252, is supposed to have been heated all to a uniform temperature and then kept in a stream of cold water at constant temperature. The exisothermal curves P A A A, P B B B, P C C C, P D D D, P E E E, P F F F (Pl. X. fig. 6) represent respectively the time-fall of temperature of points situated—

at the centre of the ball,
at one fifth of the radius from the centre,

at two fifths	,,	,,	,,
at three fifths	,,	,,	,,
at four-fifths	,,	,,	,,
at five fifths	,,	,,	,,	—that is,

    at a point on the circumference.

In this figure (6), O P represents the initial excess of temperature of the whole ball over that of the stream of water, and O Q, measured along the axis of time, represents 2500 seconds from the commencement of the cooling.

The curves G G G, H H H, I I I, J J J, K K K, L L L, fig. 7, represent respectively the distribution of temperature from the centre to the circumference of the ball at times 100, 500, 1000, 1500, 2000, 2700 seconds from the commencement of the cooling, O T representing the initial difference between the uniform temperature of the ball and the stream of water, and O R, measured along the axis of distance from the centre of the ball, representing the radius.

We have to thank three of our students, Messrs. Asano, Fujioka, and Nakano, for assistance rendered in the experiments, and three others, Messrs. Nakahara, Nobechi, and Oshima, for aid given us in the calculation of the Table of the values of $\dfrac{\alpha}{\tan \alpha}$ for different values of α.

January 1878.

XXXVII. *On the Correction of the Compass in Iron Ships without use of a Fixed Mark.* By Sir G. B. AIRY, *K.C.B., Astronomer Royal* *.

[Plate XI.]

I AM indebted to Sir William Thomson for the suggestion that the Compass in an Iron Ship might be corrected for the effects of the Permanent or Polar Magnetism of the ship without use of a fixed mark. On considering the subject, the process here described quickly suggested itself. It is based upon the following assumptions :—

That the effect of the Transient Induced Magnetism may be neglected.

That by means of an auxiliary compass the ship's head may be kept steady on one bearing for a few minutes.

That the magnitude of the actual directive force may be ascertained, in terms of any arbitrary scale (the most con-

* Communicated by the Author.

venient expression will probably be, the square of the number of vibrations made in one minute of time), by the vibration, either of the compass-needle if very finely mounted, or by the vibration of a needle suspended by a silk fibre, the compass being removed during this observation.

It is almost unnecessary to say that the apparent bearing of the ship's head as referred to the disturbed compass, or rather the bearing of the disturbed needle as referred to the head-and-stern line of the ship, is to be observed.

The circle represented in the accompanying diagram, Pl. XI. (which, in practice, ought to be made from an engraving, in order that a separate circle may be used for each separate correction of compass) is to be conceived as attached to the ship's deck, with the line H S parallel to the ship's keel.

Then the operation proceeds as follows :—

The ship's head is to be placed in three different azimuthal directions ; the most favourable will be three directions which very rudely divide the horizon into three not very unequal azimuths. In each direction, the magnitude of the actual directive force, and the bearing of the disturbed needle as referred to the head-and-stern line, are to be observed.

In the first observation, let A O represent the magnitude and direction of the actual directive force ; in the second and third observations, let B O and C O represent similar quantities. Take the metallic T-square represented at the bottom of the diagram ; apply its graduated edge to the points B and C so that the numerations of the graduations touching B and C are equal (the scale of the graduations is unimportant, all that is required being that they be equal on the right side and on the left side), and draw a pencil along the fiducial edge of the transverse arm, thus describing the line b c, b c. Apply the graduated edge in like manner to the points A and C, and thus describe the line a c, a c, intersecting the former line at P. The same operation may be performed on A and B, describing the line a b, a b ; but it is not required, as that line will necessarily pass through the point P.

Then O P represents in direction and in magnitude (on the same scale as A O, B O, C O) the magnetic force which must be introduced for the correction of the compass. For, it is a force which accompanies the ship in all its motions ; and its introduction, and its composition with the observed forces A O, B O, C O, will produce the resultant directive forces A P, B P, C P ; which, from the nature of the geometrical process, are equal, and will represent the terrestrial directive force, equal in magnitude for the three positions of the ship.

The actual operation of correction will be the following :—

1. The length of the correcting magnet must be parallel to O P. If it is to be applied end-on, it must be in the line O P. If it is to be applied broadside-on (which is preferable) draw the line *m m* through O and at right angles to O P : the centre of the magnet must be in that line.

2. Join P C, and if necessary produce O C to Q. Draw O R parallel to P C. Then Q O R is the angle through which the direction of the compass-needle is to be changed by the application of the correcting-magnet ; and the distance of that magnet is to be changed (always preserving its direction, as already described) till the compass-needle points in the direction R O. Instead of C, A or B might have been used in the same way.

Royal Observatory, Greenwich,
March 11, 1878.

XXXVIII. *On Permanent Plateau's Films.*
By Silvanus P. Thompson, *B.Sc. B.A.**

1. THE film-figures, which occupy so large a part of the researches of Plateau[†] upon the Molecular Statics of Liquids, when prepared with the glyceric fluid prescribed by their discoverer, are of extreme fragility and of short duration. With such a liquid films have been made which lasted ten, twelve, or even sixteen hours in the air, and from fifteen to thirty hours when protected by an external vessel of glass. In one instance[‡], where chloride of calcium had been added to the liquid, a duration exceeding fifty-four hours was observed. The average duration of the films, especially if they are to be exhibited to a number of persons, is more brief.

No method hitherto described of producing these films in a more durable or permanent form has been quite satisfactory, though there have been several attempts. Of these the writer was not aware when he began the present investigation, though most of them are mentioned in the later chapters of Plateau's work already named. A brief enumeration of these attempts will therefore preface a description of the process now announced for rendering the films permanent.

2. M. Plateau has himself endeavoured[§] to fix the film-figures by dipping the wire frames into solutions which eva-

* Communicated by the Physical Society.
† *Statique expérimentale et théorique des liquides soumis aux seules forces moléculaires.* Par J. Plateau. Gand et Leipzic: 1873.
‡ Plateau, *op. cit.* vol. i. p. 175, § 106.
§ Ibid. vol. ii. p. 119, § 311.

porate, leaving films of greater or less tenacity. He was unsuccessful with collodion and with albumen. A solution of gutta-percha in bisulphide of carbon gave better results. The system of films upon a cubical frame of 2 centims. side was preserved for several months, but eventually fell to powder. The same substance refused to form a film upon a frame of 3 centims. side. Glass, which in the single instance of the spherical film or bulb is so familiar, presents too many difficulties to be applicable for the production of the film-figures.

Schwartz[*] succeeded with much ingenuity in obtaining the anticlastic film-surface upon a skew quadrilateral frame whose sides were 3·5 centims. long, with gelatine.

Prof. Mach[†] imitated the system of films developed upon a tetrahedral frame with thin laminæ of caoutchouc covering the sides, and drawn together when the air was exhausted from within.

Better results have been yielded by viscous liquids which solidify at temperatures moderately low.

M. Rottier, of Ghent, has obtained films of considerable dimensions with a mixture, suggested by Böttger in 1838 for blowing bubbles, consisting of 8 parts of resin (colophony) with 1 of linseed oil, and fusing at 97°. But the films were always found after a few hours to have broken by contraction.

Mach[‡] has obtained films upon a tetrahedral frame of 5 centims. side dipped in fused resin. He has also obtained films from solutions of alkaline silicates which hardened on exposure to the air.

M. Plateau[§] has found a mixture of 5 parts of resin with 1 part of gutta-percha superior to resin alone. A system of films upon a cubical frame of 5 centims. side, prepared by M. Donny, was preserved for two years, but ultimately fell into fragments.

3. The author's first experiments were made with pure amber-coloured resin fused. The resulting films were brittle and of irregular thickness. When 10 per cent. of turpentine was added, the liquid was too mobile at high temperatures to form films, and at low temperatures too stiff to form them regularly.

A mixture of pure resin with Canada balsam was tried, with good results; and a series of experiments followed, to

* Plateau, *op. cit.* vol. i. p. 233, § 141.

† *Die Gestalten der Flüssigkeiten*. Prag.: 1872. See also Plateau, *op. cit.* vol. ii. p. 374, § 210 *bis*.

‡ *Wiener akademischer Anzeiger*, 1862, vol. xlvi. 2nd part, p. 125, "Ueber die Molecularwirkung der Flüssigkeiten."

§ *Op. cit.* vol. ii. p. 119, § 314.

ascertain the best proportions. When the mixture contained a less proportion of balsam than 35 per cent. the films were too brittle, and irregular in form. If it contained more than 70 per cent. of balsam the films did not readily harden, and were not formed without difficulty. A mixture of 55 per cent. of resin with 45 of balsam, which fused about 85°, gave good films, tough on cooling, but somewhat brittle. The mixture yielding the most satisfactory results contained 46 per cent. of resin and 54 of balsam. This mixture is sufficiently fused at 80° to be workable, but yields the best films at 93° to 95°. At 105° films can be obtained; and they are thinner than those formed from the more viscid fluid at 95°. At 110° films are still obtainable; and they frequently exhibit chromatic phenomena, but usually burst before hardening.

[The specimens exhibited to the Society are made with this mixture. They include a cubical frame of 2·5 centims. side, and a tetrahedral frame of 3·1 centims. side. Larger specimens have been obtained, however, though they generally show some imperfection of form. I have had a flat circular frame of 11 centims. diameter covered with a film of beautiful transparency. Brass wire appears better than iron for the frames.]

The films made with the mixture described are remarkably tough, and if preserved from rough handling appear to be of indefinite durability. A number of frames holding films have been hanging for over two months unprotected upon the wall of the laboratory of the writer, and are still intact. Brass wire of 0·33 millim. in diameter has been employed for the construction of the frames. When a thicker wire is used, the films become irregular from the longer retention of heat by the wire, and the consequent earlier cooling of the central portions of the films.

As with the soap-films, so with those of resinous matter, success depends largely upon the purity of material employed. Dust and oily matters must be scrupulously excluded ; and the resin should be retained at a temperature near its boiling-point for some time, to purify it of more volatile matter, before the balsam is mixed with it.

The most perfect films are obtained when the wire frames, after being dipped in the liquid, are removed to an air-bath at the temperature of about 80°, in which they are left, and the whole is allowed slowly to cool.

In proof of the toughness of the films, it may be mentioned that a recent flat film upon a circular frame of 4 centim. diameter of iron wire of 0·9 millim. gauge sustained, without breaking, the pressure of a cylindrical fifty-gramme weight, of 24 millims. diameter, placed upon its centre.

XXXIX. *On* Grove's *Gas-Battery.*
By Henry Forster Morley, *M.A., B.Sc.*[*]

IT appears to me that the question as to the mode of action of the well-known gas-battery has not yet been definitely settled.

1. The discoverer says, "The chemical or catalytic action can only be supposed to take place, with ordinary platina-foil, at the line or water-mark where the liquid, gas, and platina meet"[†]. Nevertheless he showed that water containing oxygen in one tube and hydrogen gas in the other tube gave a continuous current[‡]. As regards exp. 29 in the last-quoted excellent paper (viz. the experiment in which, hydrogen being in one tube and nitrogen in the other and no oxygen being dissolved in the liquid, hydrogen was found to appear in the nitrogen tube), as Mr. Grove does not say that there is a current, and as the presence of a current would contradict the conservation of energy, I am inclined to think that the effect is due to diffusion, and that it would occur whether the platinums were joined or not.

2. Mr. Justice Grove says that the phenomenon does not take place when the nitrogen is absent and its place filled by the liquid; and this is just what we should expect if the effect is due to diffusion. Mr. Grove thought it just possible that the hydrogen decomposed the water in its tube, combining with the oxygen, and that an equal amount of hydrogen was liberated in the other tube. Since the total amount of water is not changed, it is clear that such a decomposition could not be accompanied by a current.

3. Nevertheless Dr. Schönbein said that pure water containing no oxygen in one tube and an aqueous solution of hydrogen in the other gave a continuous current[§]. M. Gaugain makes the same assertion, but adds that he deprived his water of air by boiling[||]. To boil water and then let it stand in the air is evidently not enough to deprive it of oxygen; hence these anomalous results may be due to the water not having been absolutely free from oxygen. Such a current, as before stated, would contradict conservation of energy: indeed it has been shown by Mr. Grove that water absolutely free from oxygen in one tube and hydrogen gas in the other tube produces no current[¶].

[*] Communicated by the Physical Society.
[†] Phil. Mag. December 1842. See also Phil. Trans. 1843, p. 107.
[‡] Phil. Trans. 1843, exp. 28 &c.
[§] Phil. Mag. March 1843.
[||] *Comptes Rendus*, February 25, 1867; Phil. Mag. June 1867.
[¶] Phil. Trans. 1843, exp. 7 and elsewhere.

4. In one experiment Mr. Grove arranged his platinum plates, which I believe were platinized, in such a way as just to cut the surface of the liquid in the tubes: he got a strong current until the liquid rose above the platinum, when it became very weak. M. Gaugain says, and, I think, rightly, that this is due to the greater thickness of liquid through which the gas must now pass in order to get at the platinum—when the platinum is partly exposed the film along the line of junction being extremely thin.

5. M. Gaugain made a cell in which the platinum plates were movable, and determined, by the method of opposition, the electromotive force when the plates were partly exposed; he then lowered them until they were wholly immersed, and determined the electromotive force immediately. In this experiment the current was only allowed to flow for a few seconds. He found that the two determinations were the same, and concluded that the action of the battery depends entirely upon dissolved gas. It is, however, open to any one to assert that the platinums, when lowered, retained minute bubbles of gas on their surface, and that thus there were still many points of contact of liquid, gas, and platinum.

6. M. Gaugain, following Dr. Schönbein, asserts that " the oxygen serves simply to depolarize the positive wire," and " that its function is that of sulphate of copper in Daniell's cell "—in other words, that, were it not for the opposition current developed by the freshly-deposited hydrogen, the current could be kept up indefinitely without the presence of oxygen. As I have before stated, I cannot conceive this state of things.

I. In order to show that some, at all events, of the current in the gas-battery is due to dissolved gases, I made the following experiments in the laboratory of Professor Carey Foster:— A gas-couple with wholly submerged non-platinized platinum plates was charged by electrolysis and short-circuited for a week, after which the lengths of the columns of oxygen and hydrogen were read off by means of a telescope on different days, the couple being all the while short-circuited. A similar couple, from which the platinum plates were removed after it had been charged, was similarly treated.

The barometer-reading was, of course, corrected for expansion, for the column of liquid below the gas, and for aqueous tension, the slight effect of sulphuric acid on the aqueous tension being neglected. A correction was applied for the curved ends of the tubes, and the corrected lengths reduced to $0°$ C. 760 millims.

The result in millimetres for the couple without platinum plates was:—

	Nov. 13.	Dec. 11.	Jan. 9.
Hydrogen	76·3	76·4	76·3
Oxygen	49·9	49·7	49·7

Practically the volume of the gas in these tubes was not altered by diffusion.

For the tubes which contained the platinum plates the lengths were:—

	Nov. 13.	Nov. 20.	Dec. 11.	Jan 9.
Hydrogen	56·5	56·6	55·5	55·0
„ calculated	56·6	56·4	55·75	54·9
Oxygen		34·4	34·2	34·2

The second line is calculated from the first by least squares, on the assumption of a uniform decrease of hydrogen. The greatest error is $\frac{1}{4}$ millim.; and 1·7 millim. has disappeared. The oxygen seems to have been supplied by the air.

II. On December 11 I joined the plates through a galvanometer of 6917 ohms resistance. The connexion through the galvanometer was made without previously breaking the circuit; yet a current was instantly shown; after 19 hours the deflection was $20\frac{1}{2}$ divisions. By comparison with a Daniell's cell whose electromotive force I assumed to be 1·1, I found that a deflection of 1 division indicated a current of ·00000000056 electromagnetic unit.

If we assume that the current in the short circuit is the same as that passing through the galvanometer, an assumption which later experiments show to be not far from the truth, we shall find that 8 cubic millims. of hydrogen per week would be required to keep up this current. Now 32 cubic millims. have actually disappeared per week. The difference may be partly due to the inaccuracy of the assumption just made, and partly to the fact that some of the hydrogen combines with oxygen that has found its way from the air into the hydrogen-tube, the local currents thus produced not contributing to the main current.

III. An experiment similar to I., in which, however, the gases were prepared chemically, and in which there was also a gas-couple whose plates were not joined by a wire. The lengths of gas, in millimetres, corrected as before, were:—

For the couple with joined plates—

	May 8.	Oct. 4.	Loss.	Volume lost, in cubic centims.
Hydrogen	146·1	136·0	10·1	2·87
Oxygen	64·8	62·7	2·1	·60

For the couple with unjoined plates—

	May 8.	Oct. 4.	Loss.	Volume lost, in cubic centims.
Hydrogen	63·9	59·3	4·6	·90
Oxygen	64·1	61·3	2·8	·44

For the couple with no plates—

Hydrogen	92·0	86·9	5·1	1·30
Oxygen	90·0	85·4	4·6	·74

In this case a good deal of gas seems to have been lost by diffusion.

The ratio of hydrogen lost to oxygen lost in the three cases is 4·8, 2·1, and 1·8 respectively. If we assume that 1·8 is the ratio of the gases lost through diffusion, and that the loss of oxygen in the first two cases is due solely to this cause, we shall find that 1·79 and ·11 cubic centim. of hydrogen respectively still remain to be accounted for in the two cases. I attribute this loss to local currents in the second case, and partly to these but chiefly to the main current in the first case, most of the necessary oxygen being supplied by the air to the liquid.

IV. If the hydrogen in a gas-couple with submerged plates be warmed by the hand, the current is increased; and if it be cooled the current is diminished: indeed it is very sensitive to changes of temperature, and of pressure also; and hence it is hardly possible to determine its strength with much accuracy. The further any horizontal layer of liquid in the hydrogen-tube is from the gas, the less hydrogen does it contain. Any expansion of the gas from heat or decrease of pressure brings a more saturated solution into contact with the immersed plate and the current increases, whereas contraction produces the opposite effect.

V. When a cell has been recently charged by electrolysis the current is at first very strong; but it soon falls off, and at last remains of nearly constant strength. This is because the water was at first saturated with the gas, but this gas being used up by the current takes some time to be restored by solution at the surface, and when equilibrium is attained the liquid round the plate will contain less dissolved gas the further it is from the surface. M. Gaugain attributed the falling-off in the strength of the current to the deposition of hydrogen on the positive plate; there is no need, however, for any such supposition. I employed a battery in which the plates were wholly immersed; and the final current varied with the depth of the top of the plate in the hydrogen-tube from the surface, and with the resistance in circuit, as the following Table shows:—

n.	C.	R.	E.	C calc.
63	25	240,000	6,000	25
63	39	11,600	450	40
63	42	1,600	67	41
8	203	10,200	2,070	204
7·5	217	200	43	219
10·5	201	10,200	2,050	196
11	81	244,500	19,800	81
0	277	10,200	2,800	224
0	91	234,500	21,300	89

n is the distance between top of plate in hydrogen-tube and surface of liquid in that tube, in millimetres; $n = 0$ means that the plate cuts the surface and rises about 1 millim. above it: the plate in the oxygen tube was always at the same distance from the surface and was wholly immersed. C is the final strength of current, usually several hours after introducing resistance, given in deflections of the galvanometer, each of which is about ·00000073 weber. R is the resistance in ohms; and E is $\dfrac{CR}{1000}$.

In the first place, it seems that the increase of current consequent on causing the platinum to cut the surface is too slight to oblige us to assume that a new force is thereby brought into action; in other words, the whole of the current in the gas-battery is due to dissolved gas.

I tried to express the relation between n, C, and E in a formula obtained theoretically. In this I had but little success; but perhaps I may venture briefly to indicate the results:—

Let abscissæ represent depth below surface; ordinates quantity of hydrogen in solution at any level, supposed uniform. Imagine a tube of uniform bore open to air at D and to hydrogen at A. Suppose B C a uniform platinum rod, or rather an indefinite number of infinitely near equal platinum plates. Consider them to form equal branches of a divided circuit, and suppose the strength of current the same in each.
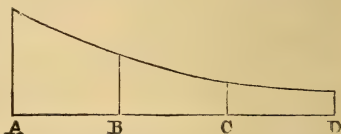
Suppose the number of molecules of hydrogen ejected from any layer in a given time to be proportional to the total number in that layer. Let u_x be the quantity of hydrogen in a layer at distance x from A; then, when equilibrium is attained, $\triangle^2 u_{x-1}$ is proportional to the number of molecules destroyed in that layer in a given time; hence it is 0 between A and B and between C and D, but it is constant between B and C; or the curve representing quantity of hydrogen is straight,

except between B and C, where it is conic. I assumed that there is no discontinuity, and that the electromotive force is proportional to the mean quantity of hydrogen in any layer between B and C, or, what is the same thing, to the total quantity between those limits, and that the current is proportional to the total quantity destroyed per second. Hence I deduced the formula

$$(1 + na)C = b - (c + nd)E;$$

where a, b, c, d are constants depending on the lengths BC, CD, on the rate of escape of gas at D &c. The layer A was assumed to be always saturated. In the actual experiment the shape of the plates was by no means regular; and even had they been quite regular, the assumption that the whole of each horizontal layer is a uniform solution is far from the truth. So I wrote the formula in a little more general form,

$$(1 + na)C = b + ne - (c + nd)E.$$

If in this we put $a = \cdot 0006$, $b = 244 \cdot 5$, $e = -3 \cdot 2$, $c = \cdot 00725$, $d = -\cdot 0000715$, we get the last column (called "C calc.") given above. We should expect the last two results to be higher than the calculated values, since no allowance has been made for the capillary film rising round the emerging platinum. However, the formula evidently cannot hold for depths much greater than 63, and it would become necessary to introduce terms varying as n^2 &c.

VI. From the above Table it is clear that the electromotive force is not constant, as in ordinary voltaic cells, but rises with the resistance. The same thing happens with ordinary gascouples with platinized platinum. In one case, changing the external resistance from 46 to 10,000 only lowered the current from 423 to 157; in another case, changing resistance from 10,000 to 190,000 lowered the current from 690 to 140.

When the resistance is suddenly increased the strength of current suddenly falls, but it rises, at first quickly and afterwards more slowly, to near its former value. For when the resistance is increased the current falls by Ohm's law; but it now uses up less gas, so that the gas accumulates in the liquid, and by so doing raises the electromotive force, and therefore the current; and this continues until equilibrium is attained.

So when the resistance in circuit is diminished the current rises suddenly, but afterwards falls to near its former value. For the current rises by Ohm's law; but the increased current uses up more gas, and so impoverishes the liquid surrounding the platinum, thereby diminishing the electromotive force, and the current falls.

These observations seem fatal to the hypothesis that the

action occurs at the junction of liquid, gas, and platinum; for the gas at that point remains of constant density, whatever the resistance in circuit may be.

VII. As an example of these views, an ordinary gas element with platinized plates was joined through a resistance of about 10 ohms, including a galvanometer. After about $1\frac{1}{2}$ hour the galvanometer was deflected 195 divisions; and after 19 hours the deflection was 189. The gas-element was now slanted at an angle of $40°$, the plates forming inclined planes; the current rose gradually, and after $2\frac{1}{2}$ hours the deflection was 235, after $23\frac{1}{2}$ hours it was 221. The element was now rotated $90°$, so that the plates were vertical, but their long diameter was still inclined at $40°$ to the horizon; the current rose instantly to 265, and after $4\frac{1}{2}$ hours the deflection was 262.

When the plates form inclined planes the line of junction between liquid, gas, and platinum is not altered; but the whole surface of the liquid is increased, and the submerged plate is brought nearer to it; hence the current is increased. In the last position an increased line of junction is added, and the current is still greater.

VIII. The current produced by the ordinary gas elements which I used was always greater when the tubes contained but little gas than when they were full of gas, the ratio being, in three cases, $1\frac{3}{4}$, 7, and 18. This is because the greater the distance between the surface of the liquid in the tubes and the air the purer will the solution of gas near that surface be. Perhaps also the greater length of the plate may enable it to catch gas that would otherwise escape: the internal resistance between the most active parts, those near the surface, would be rather increased than diminished.

However, the cells are not at all regular in their action; and this may be due to irregularities in the deposition of the finely divided platinum on the plates. These irregularities do not affect VII., since the tendency of the cell on that occasion was to become gradually weaker.

IX. M. Gaugain found that the electromotive force of platinum-wire electrodes partly exposed to the gas was not altered by submerging them. I have said why this does not appear to me conclusive (5). But I inverted the experiment: ignited wires were put several centimetres below the surface of the gases; the electromotive force was 102. They were then raised so as to be only just submerged; the force was 134. They were then thrust up into the gases, and the force was 136. A key connecting the wires through a galvanometer was pressed down until the needle had got to the end of its first swing; when the needle had come to rest the operation was

repeated; and the mean of the two swings is the number given above. The result agrees with M. Gaugain's experiment.

X. On another occasion I measured the electromotive force of wholly submerged wires in a gas-couple by connecting them to a condenser, and afterwards discharging the condenser through a galvanometer. The electromotive force of thick and thin platinum wires was the same; but this was 15 times that of a wire of gold. Probably in the gas-couple, as elsewhere, platinum exerts some specific attraction on hydrogen.

XI. M. Gaugain considers the falling-off in the strength of a gas-couple after short-circuiting to be due to the deposition of hydrogen on the positive wire, which hydrogen is produced by the decomposition of water by hydrogen; and he says that when the electromotive force of a couple fell from 152 to 30, that of the hydrogen-wire fell 26, while an antagonistic force of 96 was developed by the wire in oxygen[*]. From other experiments of M. Gaugain, I infer that the potential of each wire was compared with that of a third wire plunged in the liquid between the two tubes of the couple. He does not distinctly say that the positive wire of the couple actually became negative to the third wire, though this may perhaps be inferred from the expression "antagonistic." I consider the loss of potential to be due to the liquid near the wires becoming impoverished of gas; and even should the oxygen-wire become negative to the third wire, it may only show that the liquid in its neighbourhood contains less oxygen than that surrounding the third wire. But since a little hydrogen must have found its way into the oxygen-tube, this has a much better chance of becoming attached to the platinum when there is little oxygen near to use it up (that is, when a current is passing) than when the circuit has been broken and the wire is surrounded by a strong solution of oxygen. Using a gas-couple with wholly submerged platinum wires, and comparing these with a third wire in the liquid between the tubes by means of a condenser periodically discharged through a galvanometer, I found in two different cases, *a* and *b*, just before short-circuiting:—

	a.	*b.*
Hydrogen-wire	108	74
Oxygen-wire	12	17
	120	91

and in the same soon after breaking the circuit:—

	a.	*b.*
Hydrogen-wire	41	12
Oxygen-wire	0	12
	41	24

[*] *Comptes Rendus,* 1867.

In one case the oxygen-wire gave a negative deflection of about 1.

I found that when I ignited a platinum wire in a Bunsen's flame it acquired a positive potential of about 20, as if it had absorbed oxygen. In §§ IX., X., and XI. the cell was composed of a couple of test-tubes inverted in a beaker of acid, and the wires were introduced by pushing them through a couple of narrow U-tubes, the shorter arms of which were inside the test-tubes. This arrangement made it very easy to change the wires.

XII. The maximum polarization of a voltameter is scarcely, if at all, altered by diminution of pressure (Crova); and the same seems to hold for increased pressure. So also is the electromotive force of a freshly charged gas-couple—being, in fact, little less than that maximum polarization. I connected the terminals of a gas-couple with a condenser which could be discharged through a galvanometer; I then developed gas by electrolysis for a few minutes, during which time the difference of potential between the wires, which I will call E, was 189, the pressure being 77 centims. of mercury; the battery was then cut out, and as soon as most of the bubbles, except those sticking to the wires, had risen, I found $E = 60$. The wires were now short-circuited until $E = 34$, when the pressure was increased to 145 centims., after which the battery was put on and $E = 191$; then the battery was cut out as before, and $E = 62$; then the wires were short-circuited till $E = 37$; then the pressure was reduced to 22 centims.; then the battery was put on, and $E = 200$; after cutting out the battery, $E = 60$. The initial electromotive force of the element is not affected by the length of time the battery is in circuit.

The difference of potential between the terminals of the battery was about 260, but was slowly rising during the experiment. When hydrogen is liberated from the surface of the wire, the platinum attracts as much of it as it can: this quantity seems not to vary with the pressure; I do not know why it should so vary; and it determines the maximum polarization. The slight increase of the polarization with pressure may perhaps be attributed to changes in the density of the oxygen.

XIII. A gas-couple charged with chemically prepared oxygen and hydrogen was short-circuited through a galvanometer of 123 ohms resistance, and subjected to various pressures, the top of the wire in hydrogen being 5·7 centims. below the surface of the liquid in its tube, and that of the wire in oxygen being 5·4 centims. below the surface. At a pressure of 76 centims., deflection 15. At a pressure of 144 centims. the deflection

gradually rose in 7 hours from 12 to 26. The hydrogen-wire
being 2·1, and the oxygen wire 1·9 centim. below the surface,
at a pressure of 76 the current is 38,

     ,,   ,,   40   ,,   ,,   20 after 6 hours,
     ,,   ,,   16   ,,   ,, $7\frac{1}{4}$ after 5 hours.

Now pressure divided by current for the last three cases
gives 2, 2, 2·2 respectively; or the current is directly as the
pressure. In the last case $7\frac{1}{4}$ seems to be somewhat too low;
but this may be attributed to bubbles of oxygen, which under
the low pressure were given off in the hydrogen tube. For
the first two cases, pressure divided by current gives 5·1 and
5·5 respectively. It is possible that at the end of the second
experiment the current was very slowly rising: the further the
wires are from the gas, the longer, of course, does it take for
equilibrium to be attained.

In this experiment the gases were introduced by stopcocks
into the upper parts of the branches of a U-tube, the platinum
wires were sealed into the lower parts of those branches, and
the bend of the tube had a tail by which the pressure was ap-
plied; so that the gases were introduced without coming into
contact with the wires. The same instrument, being at hand,
was used in § XII., where it is called a voltameter.

If the action of the gas-couple depends entirely on solution,
it is natural that the current should be proportional to the so-
lubility of the hydrogen—that is, to the pressure. But if
there is really any antagonistic force kept up by hydrogen
attached to the positive wire, we should expect that this force
would not be altered by pressure, and so the whole current
could not be proportional to the pressure. I suppose that when,
by increased pressure, the electromotive force becomes equal
to the maximum polarization, further increase of pressure would
not alter the current.

. The Physical Laboratory,
 University College, Londo
    December 1877.

XL. *On some Physical Points connected with the Telephone.
By* WILLIAM HENRY PREECE, *Vice-President of the Society
of Telegraph Engineers, Memb. Inst. C.E., &c.*[*]

THE introduction of the speaking telephone, by Alexander
Graham Bell, has supplied physicists with an instrument
of research as well as with an instrument of practical utility.
It is an apparatus which, for the examination of certain kinds
of currents of electricity, is the most delicate that has yet been
invented. Indeed it has rendered evident the presence of cur-

    [*] Communicated by the Physical Society.

rents whose existence, though suspected, have hitherto eluded the grasp of the electrician ; in fact its very delicacy has proved the greatest obstacle to its general adoption.

I. *The Telephone as a Source of Electricity.*

Faraday showed that, when a closed conductor is moved across the lines of force in a magnetic field, a current of electricity is generated within that conductor whose strength is dependent upon the velocity of motion of the conductor and upon the intensity of the magnetic field. It is, in fact, proportional to the number of lines of force cut through per unit of time. And also, when lines of force are projected through a closed conductor, a current of electricity is generated in that conductor, whose strength depends upon the magnetic intensity of those lines of force, or upon their number per unit area. The direction of the current in each case is found by Lenz's law, viz. that the current produced tends to resist the motion producing it. The new principle that has been developed by Professor Graham Bell is that the form and duration of that current is dependent upon the rate and duration of the motion of the moving body or of those lines of force.

Let N S, fig. 1, be a permanent magnet, and $a\,b$ a fixed, closed, conducting ring of copper around one pole of that magnet. Let c be a movable iron armature. Now, if we regard any two lines of force 1 radiating from the pole N, and nearly cutting the ring $a\,b$, then, as we make c approach or recede from N, those lines of magnetic force will change their direction,

Fig. 1.
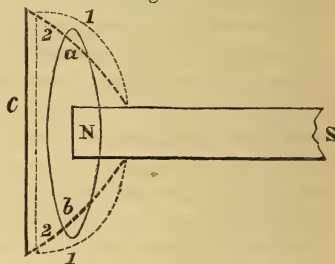
taking up position 2 ; and with each change of direction they will cut the ring $a\,b$, and currents of electricity in different directions will circulate through $a\,b$ according to the direction of motion of the lines of force ; and the rate of increase and decrease of magnetic intensity (or of the increment and decrement of the current) will vary directly with the rate of motion of the armature c to or from the pole N. Thus, if c be a disk of iron vibrating under the influence of sound, the excursions to and fro of any point of the disk, though very small (in fact they are so small that they can scarcely be detected by the most delicate means—so small that they have led Graham Bell to imagine that the vibrations are molecular), are nevertheless sufficient to produce that motion of the lines of force which results in currents. It is, however, a fundamental principle

in acoustics, that wherever there is sound there is always vibration. Sound and vibration are concomitant and inseparable phenomena. The air cannot produce sound unless it is thrown into vibration ; and the air itself cannot be thrown into vibration unless the mass of matter in contact with it vibrates also. The amplitudes of vibration of the particles of the air themselves ⌐ have never been measured, though the length of a sound-wave (a very different quantity) is accurately known. Lord Rayleigh has shown that an amplitude of only $\frac{1}{10000000}$ of a centimetre is sufficient to produce sonorous vibrations. But though the amplitude of the vibrations be so small they are rapid. Now this rate of motion is sufficient to bend in the same ratio the lines of force cutting $a\,b$, and thereby to produce currents of electricity in the ring $a\,b$ whose number depends on the number of vibrations, and whose form and intensity depend on the rate and amplitude of motion of the disk c. These currents are alternate, and so rapid that no known instrument but the telephone indicates them ; but they are readily shown by a Thomson's reflecting galvanometer when the disk is gently and slowly pressed in by the finger,—in one direction when the disk is pressed in, in the other direction when the disk is allowed to fly back.

I have failed hitherto to make even an approximate measurement of their minuteness. We have no known standard to compare them with : we can only trust to the ear ; and that instrument is not only deceptive but variable. They are certainly less than $\frac{1}{1000000}$ of an ordinary working current. Mr. R. S. Brough, of the Indian Government Telegraph Department, has calculated that the strongest current with which a telephone is at any moment worked does not exceed $\frac{1}{1000000000}$ of the C. G. S. unit, or *weber* ; and Professor Pierce, of Boston, found that similar effects are produced with an electromotive force of less than $\frac{1}{200000}$ of a volt or Daniell's cell. Thus we have a source of electricity competent to produce currents of microscopic strength, which vary in form, duration, and intensity with the motion of the body producing them.

II. *The Telephone as a Detector.*

Fig. 2.

Let $n\,s$, fig. 2, be a core of soft iron surrounded by a closed conductor $a'\,b'$, through which currents flow. Now this core will become magnetized with an intensity dependent solely upon the intensity of the current ; and the intensity of magnetism at any moment will be a function of the intensity of the current at that moment ; so that if the current increase and decrease with a given ratio
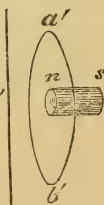
and at a given rate, the intensity of magnetism will increase and decrease with the same ratio and at the same rate. The disk c' is elastic, but it is rigidly fixed at its axis; it being of iron, it is attracted at any moment with a force dependent upon the intensity of the magnetism of the core n s, and being elastic, it recovers, or tends to recover, its normal position whenever this intensity of magnetism ceases or diminishes. Thus, if the magnetic intensity varies, the force of attraction varies, and the rate of motion of the disk varies in the same way. Hence the disk will record exactly the variations of the currents; and as the currents are the result of the variations of the vibrations of another disk, the one disk c' simply repeats exactly the vibrations of the other disk: thus sounds are reproduced.

Though in the earlier instruments the coil surrounded a pole-piece of soft iron, this pole-piece has since been discarded, and the coil surrounds the pole of the magnet itself. The efficacy of the instrument has been in no way impaired by this change; and it has the additional advantage of being perfectly reversible, the same instrument being used for speaking and for hearing.

III. *Working the Telephone.*

There is a remarkable difference in the power of different voices to work the telephone. Shouting is of no use. The intonation must be clear and the articulation distinct, and the style of conversation approach more the sing-song. I have heard Mr. Willmot, one of the electricians of the Post Office, through resistances that have drowned all other voices. The vowel sounds always come out the best; the palatal sounds c, g, j, k and q, the worst; in fact, the latter sounds are frequently lost. The ear also requires a certain education; and the power of hearing varies surprisingly with the different ears and with different people. Singing always comes through with remarkable distinctness; and the sounds of a wind-instrument—the cornet or the bugle—are reproduced with startling force. A bugle sounded in London was heard distinctly over the large Corn Exchange of Basingstoke by a thousand people. This arises from the regularity as well as increased amplitude of the sonorous vibrations, and consequently from the regularity, uniformity, and increased strength of the currents of electricity.

IV. *Improvements.*

Every one who has the means at his disposal has been endeavouring to increase the power of this instrument. I should be sorry to enumerate the number of experiments I have

tried, but all with vexatious, disappointing, and dispiriting failure.

One of the earliest efforts was made by Mr. Willmot, who hoped by increasing the number of diaphragms, coils, and magnets acted upon simultaneously, and joining up all those coils in series, to obtain a resultant effect that would magnify the out-going currents; but the result showed that, while the apparatus acted all right, the effect of displacement of each diaphragm decreased with their number, and the ultimate effect was the same as with one diaphragm. Mr. Willmot's instrument, which was made early in October last, is on the table ; M. Trouvé, in Paris, seems to have been working on the same idea.

Increasing or varying the size, form, and strength of the magnet has produced little or no apparent improvement; for the resultant effect in all cases remained apparently the same.

The greatest effect is produced with a compound horseshoe magnet, which is indeed one of the earliest forms brought out by Mr. Bell. Here we have two coils, utilizing the maximum number of lines of force ; and the effects produced are certainly the finest I have yet experienced. At Southampton, on the 14th inst., in a small office, Mr. Willmot's voice (he was in London) was heard distinctly by the seven or eight persons who were in the room at the time. Though I have made one with the largest and most powerful magnet I could obtain, the result has been as disappointing as in the previous cases. The telephone has certainly been brought to this country by Mr. Bell in almost its perfect theoretical form ; he is still labouring to improve it; and I am sure we all wish him success.

V. *Applications.*

However small and however sudden the currents may be, the telephone records them with great accuracy ; no known form of galvanometer or galvanoscope will do so.

It is admirably adapted for showing the currents of induction set up in contiguous coils or contiguous spirals. If reversals or intermittent currents be sent through one spiral while the other be gradually removed away, the rapidly diminishing effect of increased distance is very evident ; indeed all the phenomena of magneto-electric induction are strikingly shown by its means. It is also admirably adapted as a detector in the bridge of a Wheatstone's balance to test short lengths of wire, and it will probably enable us to obtain a closer approximation to equality than we have yet secured ; it also enables us to adjust condensers with great accuracy.

M. Niaudet, of Paris, has shown how it can be utilized to

detect the presence of extremely feeble currents from doubtful sources of electricity. If currents from the supposed source be rapidly sent through one wire of a double-wound coil, and a telephone be fixed on the other running parallel to it, then the telephone would give evidence of their presence, which would be indiscernible on any other instrument.

It is admirably adapted also for testing leaky insulators and supports.

VI. *Inferences and Results.*

The telephone explodes the notion that iron takes time to magnetize and time to demagnetize. If time were occupied in magnetizing, notes would be changed or lost; but they are not altered. The notion of time is due to the action of induction in coils producing *reaction* and *extra currents*. This is proved by the insertion of an electro-magnet or of coils of wire in a telephonic circuit. While it is possible to speak through a cable 100 miles long laid out straight in the sea, it is impossible to speak through 20 miles when coiled in a tank.

Its delicacy has detected the presence of currents in wires contiguous to wires conveying currents, which have always been suspected, but have been evident only on wires running side by side for several miles (say two hundred) on poles or in well insulated cables. In fact, the most delicate apparatus has hitherto failed to detect the presence of these currents by induction in short underground wires; but the telephone responds to these currents when the wires run parallel for only a few feet. Thus, between one floor and another floor, at the General Post Office, it has been impossible to converse by means of the telephone through a wire, owing to the presence of these currents of induction from the innumerable working wires contiguous to it, and through some of the underground pipes of the streets of London sounds are inaudible when the wires are working. In fact, two small-sized gutta-percha wires, *one foot long*, were lashed side by side by Mr. Marson; and when battery currents were sent through one, induction currents were distinctly heard on a telephone fixed on the other. Indeed this induction between wire and wire, has proved the most serious obstacle to the practical introduction of the instrument. But it is not altogether irremediable on underground wires; it can be surmounted in three ways:—

1. By increasing the intensity of the transmitted currents so as to overpower the currents of induction, and by reducing the sensitiveness of the receiving apparatus so as to make the instrument insensible to currents of induction though responsive to telephonic currents.

2. By screening the wire from the influence of induction.

3. By neutralizing the effects of induction.

1. Mr. Edison in America has partially succeeded in effecting the first cure ; but his results, though promising, have not yet reached a practical point.

2. I have overcome the second difficulty in a way that will now be described.

Let 1, fig. 3, be a wire used for telephonic purposes, and 2

Fig. 3.
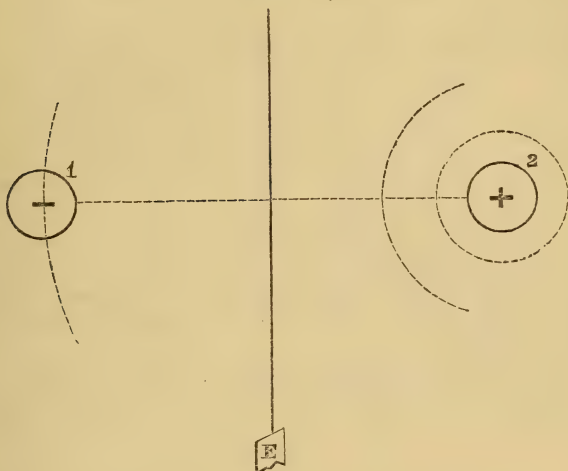
be an ordinary telegraphic wire contiguous to it. Let us regard 1 and 2 as symmetrical and contiguous particles of the two wires. If a current flow through 2 it will affect 1 inductively both statically and magnetically. Let us regard the static effect first. If the current flow away from us, then we may consider the particle 2 as charged *positively* ; lines of electric force will radiate all around it, and that line which passes through 1 will inductively charge that particle *negatively*. This influence being felt all along the wire, a current in the reverse direction to that in 2 will flow through 1. The reverse would occur if we assumed the primary current to flow in the other direction. Hence, an induced current will flow through 1, whenever the current in 2 commences and whenever it ceases. Now, if we place between 1 and 2 a screen of metal, or other conducting matter, in connexion with the earth, then the line of electric force, instead of passing through 1, will terminate at the screen. Hence, if we surround the wire 2 with a covering or sheath of metal, or if we submerge it in

water, all effects of static induction will cease between 1 and 2. In water they are not entirely eliminated, for water is a very poor conductor ; but they are so reduced by its influence, as my experiments between Manchester and Liverpool and between Dublin and Holyhead have shown, that, if the water or wet serving had been a perfect conductor, they would have been removed as far as regards static induction.

But we have to regard magnetic induction as well. Besides establishing a field of electric force around 2, a current flowing through that wire establishes a magnetic field around it, whose lines of force are circles, and whose directions are at right angles to the lines of electric force. Let us regard that line of force cutting 1. Each time a current commences, and each time it ceases, in wire 2, a line of magnetic force cuts wire 1, and produces in that wire a current of induction in the same direction as that produced by static induction. Now, if we make the screen of iron, those lines of force terminate in the iron and wire 1 is freed. Hence, if we sheath the wire 1 with iron, it is not only freed from the effects of static induction by being surrounded by a conductor in contact with the earth, but it is shielded from the effects of magnetic induction by its sheath of iron. Hence both effects of induction are entirely removed.

3. They can be neutralized by means of a return wire, using this return wire instead of the earth. If 1 and 2, fig. 4, be

Fig. 4.
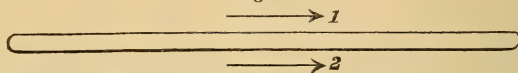
two wires running side by side, then the current set up by induction from neighbouring wires in one wire is neutralized by the currents set up in the other side.

But this assumes either that the disturbing wires are at an infinite distance from 1 and 2, or that 1 and 2 are infinitely near each other. All attempts to use return wires on existing poles, in cables, or in underground wires have utterly failed to do away with inductive disturbance ; but Mr. Bell has had a single gutta-percha wire carrying two conductors made which very nearly fulfils the conditions and gives excellent results.

The extreme delicacy of the instrument has introduced a disturbance from another cause, viz. leakage. Wires on poles are supported by glass, porcelain, and earthenware insulators ; but the best support that was ever devised is but a poor insulator in wet weather. Currents escape over their surface from the wire they support ; and these leakage currents find

their way into telephonic circuits. Hence a telephone circuit which may work well in dry fine weather will prove absolutely unworkable in wet weather.

Another source of trouble arises from what are technically called " bad earths." It is almost impossible to make a perfect connexion with the earth. There is always some resistance at that point; so that if two wires terminate on the same earth-plate, the one being a working circuit and the other a telephone-circuit, some currents from the former are sure to pass through the latter and disturb the telephone. A return wire perfectly cures this evil.

There are other disturbing elements that are peculiar. Earth-currents, which are always present in the wires, produce a peculiar crackling noise, similar to that produced by a current from a single fluid battery such as a Smee or a Leclanché, not unlike the rushing of broken water. This is due to the polarization of the earth-plate, as the sounds produced by a battery-current are due to the polarization of the negative plate. When auroras are present these earth-currents become very powerful, and the sounds are much intensified. The effects of thunderstorms are very peculiar : a flash of lightning, even though so distant as to be out of sight, will produce a sound ; and if it be near enough to be only sheet lightning, it produces, according to Dr. Channing, of Providence, a sound like the quenching of a drop of melted metal in water, or the sound of a distant rocket. Moreover he says that this sound is heard before the flash is seen, proving the existence of some inductive effect in the air prior to the actual discharge. The telephone thus becomes an admirable warning of the approach of a thunderstorm.

Sometimes a peculiar wailing sound is heard, which an imaginative correspondent of mine likened to " the hungry cry of newly-hatched birds in a nest." I am inclined to think that it is due to the swinging of the wires across the magnetic lines of force of the earth. It is not difficult to conceive that these vibrations may succeed each other in the necessary rhythmic order to produce musical tones.

The wires are never free from sound ; and every change of temperature or of the electric condition of the atmosphere is recorded on this delicate apparatus.

The expansion of the iron diaphragm under the influence of the warm and damp breath when the telephone is first raised to the lips preparatory to talk is very marked ; it produces a faint rustling shiver.

Immediately on the introduction of the instrument, great anxiety was felt to learn its performance on submarine cables.

A telephone was sent to Guernsey, and Mr. Willmot went to Dartmouth, those two places being connected by a cable 60 miles long.   Conversation was carried on, the articulation being perfect though slightly muffled.   This was a surprise; for it was felt that the static induction of a cable, by its retarding influence, would have prevented articulation by lengthening the waves of electricity and rolling them up as it were. Through the kindness of Messrs. Latimer Clark, Muirhead, and Co. I was able to repeat these experiments on an artificial Atlantic cable, constructed to duplex the direct United-States cable.   With Mr. Willmot at one end and myself at the other, there was no difficulty in speaking up to 100 miles, though the muffling effect of induction was evident.   Beyond this distance up to 150 miles muffling commenced to seriously impede conversation, and the sounds diminished considerably in strength : it was like talking through a thick respirator. The effect diminished rapidly up to 200 miles, beyond which articulation became impossible, though singing was distinctly heard ; indeed singing was heard through the whole length of the cable, 3000 miles long ; but this was traced to a secondary cause, it being due to the induction of condenser on condenser. Nevertheless there is no doubt that singing can be heard through a much greater length than speaking, due to the greater regularity of the successive waves of electricity.

I subsequently experimented on the underground wires between Manchester and Liverpool, a distance of about 30 miles ; and through this length we had no difficulty whatever in speaking.   Again, between Dublin and Holyhead, through the cable 67 miles long, we spoke with ease, singing coming through with remarkable power and effect.   This cable contains 7 distinct conductors.   When one wire was used for the telephone, the sounds could be heard on every other wire, but in a feebler degree.   When the other wires were working with the ordinary telegraphic apparatus, induction was evident, but not sufficiently intense to stop conversation.   Each wire would be surrounded with a wet serving of hemp ; but this was not of sufficient conducting-power to entirely screen the effect of induction.   The same effect was experienced between Manchester and Liverpool, where the wires are made up into cables of 7 conductors served outside with tarred hemp.

The conclusion that I have come to is, that conversation might be held through a single wire cable 200 miles long with the apparatus that we now have ; what new apparatus will do no man is rash enough to predict.

The reason for this surprising result is not difficult to explain :—

Let the disk d, fig. 5, be impressed forwards by a sonorous vibration, it will generate in the coil c a positive current,

Fig. 5.
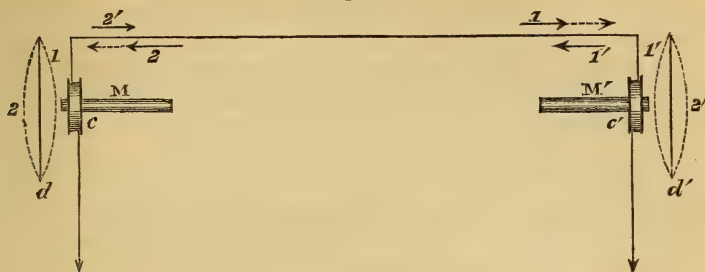
which, flowing through the line, will pass through the coil c' and attract the disk d'. Now the effect of induction is to retard or prolong the effect of the positive current 1; but the motion of the disk d' has itself produced a current in the reverse direction to the first current; and this neutralizes the prolongation due to induction, and so helps to clear the line for the next signal, which passes through precisely the same process; and hence the vibrations of the second disk tend to produce currents which diminish materially the effects of induction, and so render possible conversation to distances that far exceed anticipation.

The extreme delicacy of Bell's apparatus has been shown in various ways; for instance:—

Extract from a letter from T. A. EDISON, *dated November* 25, 1877.

"I made a pair of telephones that work with copper diaphrams: it is on the revolving-copper-disk principle of Arago.

"I find that a copper diaphragm may replace the iron in Bell's. Copper must be $\frac{1}{32}$ in. thick. It is very low with copper in both; but if the receiver is one of the regular kind, and the transmitter is a copper diaphragm, you can carry on conversation with ease both ways; but with the pair I have made the talking is loud, as I have several dodges on it."

I repeated these experiments; but the effect was so feeble as to be scarcely distinguishable, and, although interesting from a scientific point of view, it was of no practical value.

Mr. James Blyth has independently repeated the experiment, and has shown that wood, paper, and india-rubber produce similar effects. These effects are probably due to the fact that diamagnetic bodies have a similar though feebler

influence, in varying the direction of lines of magnetic force, to magnetic bodies.

Again, I have spoken distinctly and easily with telephones without any permanent magnet whatever, the core of the coil being of soft iron ; but this effect was probably due to the impurity of the iron, residual magnetism remaining in it. Dr. Blake, of Boston, has spoken easily when the core was a piece of soft iron placed in the direction of the dip.

XLI. *On the Nebular Hypothesis.*—VII. *Undulation.* By PLINY EARLE CHASE, *LL.D., S.P.A.S., Professor of Philosophy in Haverford College.*

[Continued from vol. iv. p. 298.]

THE combined influences of æthereal or *quasi*-æthereal action and reaction, elasticity, density, and fundamental velocity, in the arrangement of the Solar System, are shown by the symmetrical formula

$$\left(\frac{\mu + \mu_1}{\mu_1}\right)^{\left(\frac{\lambda_1 + \lambda}{\lambda} = \frac{\tau + \tau_1}{\tau_1}\right)} = \frac{\rho_1 + \rho}{\rho}\Big); \quad \dots \quad (1)$$

and by the equations,

$$\frac{(n\pi)^2 \sqrt{fr}}{4} = \lambda, \quad \dots \dots \dots (2)$$

$$\left(\frac{\delta_2}{\delta_1}\right)^3 \sqrt{f_1 r_1} = \lambda, \quad \dots \dots \dots (3)$$

$$\frac{n}{n_1} = \frac{\delta_1}{\delta}, \quad \dots \dots \dots \dots (4)$$

$$\frac{\delta}{\rho} = \frac{\lambda}{\lambda_1} \times 2^{\frac{2}{3}}, \quad \dots \dots \dots (5)$$

$$\frac{\mu}{\mu_2} = \left(\frac{\lambda}{\lambda_1}\right)^2 \times 2^{\frac{3}{2}}, \quad \dots \dots \dots (6)$$

$$\frac{\tau_2}{\tau_3} = \frac{\lambda}{\lambda_1} \times 1 \cdot 061. \quad \dots \dots \dots (7)$$

In these equations μ = mass of Sun ; μ_1 = mass of Jupiter ; μ_2 = mass of Earth ; λ = average velocity of complete solar dissociation = $214 \cdot 86\,\rho \div 497 \cdot 825$ = velocity of light ; $\lambda_1 = 2\sqrt{g\rho}$ = 2 × velocity of incipient dissociation at Sun's surface = mean

radial velocity of complete solar dissociation

$$= \frac{4 \times 214 \cdot 86^{\frac{3}{2}} \pi \rho}{\text{number of seconds in 1 year}};$$

τ = time of oscillation through major-axis equivalent to Sun's possible atmosphere, or to $\frac{1}{3}$ of Earth's radius vector; τ_1 = time of Jupiter's revolution; τ_2 = time of Earth's revolution; τ_3 = time of Earth's rotation; ρ = Sun's equatorial radius; ρ_1 = Jupiter's projectile radius, or mean perihelion distance from Sun; n = special coefficient of Earth's dissociation velocity ($n\pi\sqrt{fr}$); n_1 = special coefficient of Jupiter's dissociation velocity ($n_1\pi\sqrt{f_1r_1}$); δ = Earth's mean distance from Sun; δ_1 = Jupiter's secular perihelion distance from Sun; δ_2 = Uranus's mean distance from Sun; $2^{\frac{2}{3}}$ = relative radius of revolution for $2\tau_a$; $2^{\frac{3}{2}}$ = relative time of revolution for $2r_a$; $1\cdot061$ = Jupiter's $\dfrac{\text{secular aphelion}}{\text{mean}}$ radius vector.

To illustrate the closeness of the accordances, if we substitute in (1) the actual values, viz. $\lambda = \rho \div 2\cdot317$, $\lambda_1 = \rho \div 344\cdot15$, $\rho = 1$, $\rho_1 = 1069\cdot62$, the equation reduces to

$$\left(\frac{\mu + \mu_1}{\mu_1}\right)^{1\cdot0029} = 1070\cdot62; \therefore \frac{\mu + \mu_1}{\mu_1} = 1049\cdot24.$$

Bessel's estimate is 1048·88, differing from the theoretical value only $\frac{1}{29}$ of one per cent.

The significance of Earth's position, at the centre of the belt of greatest condensation, of the positions of Jupiter and Uranus, as inner planets of the two exterior two-planet belts, of the masses of Sun, Jupiter, and Earth, as the principal masses, in the system, in the extra-asteroidal and in the intra-asteroidal groups, and of the many important relations to the limiting velocity of luminous undulation, is thus clearly shown.

In the æthereal waves which are generated by the two controlling masses, μ and μ_1, we may naturally look for harmonic interferences, not only in the solar spectrum, but also in elementary molecular groupings and in cosmical masses. If we compare μ and μ_1 at Jupiter's present perihelion, we find that the product of Jupiter's radius vector by its mass is 1·0153 times the product of Sun's radius by its mass. Representing 1·0153 by a, and taking $c = b(a-1) = 0\cdot918$, we may form the harmonic progression $\dfrac{1}{a+c}$, $\dfrac{1}{a+2c}$, $\dfrac{1}{a+3c}$, &c., thus obtaining the following nodal divisors and approximations, in millionths of a millimetre, to wave-lengths of Fraunhofer lines:—

Denominators.	Nodal divisions.	Quotients.		Observed.
1	...... 1·0000	761·20	A	761·20
$a+c(f)$	...... 1·1071	687·56	B	687·49
	[1·1530	660·19]	C	656·67
$a+2c$	...... 1·1989	634·92		
$a+3c(f)$	...... 1·2907	589·76	D	589·74
$a+4c$	...... 1·3825	550·60		
	[1·4437	527·26]	E	527·38
$a+5c$	...... 1·4743	516·31	b	517·70
$a+6c(f)$	...... 1·5661	486·05	F	486·52
$a+7c$	...... 1·6579	459·13		
$a+8c$	...... 1·7497	435·05	G	431·03
	[1·7650	431·27]		
$a+9c$	...... 1·8415	413·37		
	[1·9180	396·87]	H	397·16
$a+10c(f)$	...... 1·9333	393·73	H	393·59

The harmonic interferences which are indicated by the series marked (f) are the most interesting, both on account of the close accordance between the theoretical quotients and the corresponding observed values, and because the successive denominator-increments are figurate. Moreover the figurate series (1, 3, 6, 10) is the same as I pointed out in my equation of products of triangular powers, which is applicable both to vector radii and to masses:—

$$\Psi^{\,1} \times \delta^{\,3} \times \mathit{2}\kern-0.3em\mathit{4}^{\,6} \times \hbar^{\,-10} = 1^*.$$

Of the six remaining lines, three (A, b, G) approximate so closely to the corresponding harmonic quotients, the greatest deviation being less than one per cent., that they may be properly regarded as illustrations of secondary interferences. The bracketed divisors and quotients indicate tertiary harmonics, based on denominative differences of $c' = a - 1 = ·0153$, $1·1539 = 1 + 10c'$, $1·4437 = 1 + 29c'$, $1·7650 = 1 + 50c'$, $1·9180 = 1 + 60c'$. The greatest difference between the theoretical and observed values is less than $\frac{5}{9}$ of one per cent.; all the other differences range between $\frac{1}{13}$ and $\frac{1}{44}$ of one per cent.

Among the subordinate spectral lines there are some which are closely represented by the quotients of 761·20 by the denominators $a+2c$, $a+4c$, $a+7c$, $a+9c$. But, on account of the great number of faint lines, such accordances are less satisfactory than those which can be found in the lines that are more widely separated and more prominent.

* Phil. Mag. for June (Supplement) 1876.

The following Table shows that, in planetary aggregation, the interference-waves have manifested their influence most strikingly at luminous intervals. The denominators are exponential, indicating roots which are to be extracted, instead of divisions which are to be made. This is a natural consequence of condensation in and through an elastic medium. It will be noticed that the first six exponential denominators are arithmetical means between the nodal divisors in the foregoing Table, and that the others are formed by successive denominator-increments of $\frac{5}{4}c$.

Exponential denominators.	Roots.	Observed.	
1·0000	6453	6453	Neptune.
1·0536	4130	4122	Uranus.
1·1530	2015	2050	Saturn.
1·2448	1150	1118	Jupiter.
1·3366	708	728	Freia.
1·4284	465	473	Flora.
1·5202	321	327	Mars.
1·6350	214	215	Earth.
1·7497	150	155	Venus.
1·8644	111	110	Ven.-Mer.
1·9792	84	·83	Mercury.
2·0939	66	64	,, s. p.
2·2087	53	53	,, c. o.

The observed values are the mean planetary vector radii, in units of Sun's radius. " Ven.-Mer." is the arithmetical mean between Venus's mean distance (155) and Mercury's secular perihelion (64). Mercury " c. o." is the centre of spherical oscillation ($\sqrt{\cdot 4}$) of a nebula extending to Mercury's mean distance.

The following comparisons show a few of the many harmonies which are found in the prominent lines of chemical elements. The wave-measurements in all the spectra, both solar and chemical, are taken from the papers of Prof. Wolcott Gibbs, in the 'American Journal of Science', second series, vols. xliii., xlvii. Kirchhoff's lines are indicated by K ; Huggins's by H ; Gibbs's groupings of corresponding lines in the groupings of both Kirchhoff and Huggins by KH—the left-hand columns containing Kirchhoff's estimates, and the right-hand columns those of Huggins:—

Mercury, KH.

Wave-lengths.			Quotients.		Theoretical.	
568·47	568·55	......	1·0000	1·0000	1·0000	1
546·33	546·13	......	1·0407	1·0411	1·0406	$1 + 6a$
542·80	542·80	......	1·0473	1·0484	1·0474	$1 + 7a$

Lead, KH.

561·29	561·46	......	1·0000	1·0000	1·0000	1
537·71	537·85	......	1·0439	1·0439	1·0440	$1 + 3a$
439·07	438·93	......	1·2784	1·2792	1·2784	$1 + 19a$

Ruthenium and Iridium, K.

635·45	......	1·0000	1·0000	1
545·44	......	1·1650	1·1646	$1 + 5a$
530·52	......	1·1973	1·1975	$1 + 6a$

Chromium, K.

541·35	......	1·0000	1·0000	1
521·20	......	1·0387	1·0387	$1 + 111a$
520·98	......	1·0391	1·0391	$1 + 112a$
520·83	......	1·0394	1·0394	$1 + 113a$

Arsenic, KH.

617·54	617·67	......	1·0000	1·0000	1·0000	1
611·69	611·67	......	1·0096	1·0098	1·0093	$1 + a$
578·95	578·73	......	1·0667	1·0673	1·0650	$1 + 7a$
533·55	533·41	......	1·1566	1·1580	1·1579	$1 + 17a$

Magnesium, K.

518·73	......	1·0000	1·0000	1
517·64	......	1·0021	1·0020	$1 + 2a$
517·17	......	1·0030	1·0030	$1 + 3a$
459·62	......	1·1286	1·1285	$1 + 9b$
448·57	......	1·1564		
448·39	......	1·1569	1·1570	$1 + 11b$

Zinc, KH.

636·99	637·37	......	1·0000	1·0000	1·0000	1
610·64	610·89	......	1·0432	1·0442	1·0390	$1 + a$
589·90	589·90	......	1·0798	1·0805	1·0781	$1 + 2a$
472·25	471·98	......	1·3488	1·3504	1·3513	$1 + 9a$

Cadmium, KH.

Wave-lengths.			Quotients.		Theoretical.	
647·22	647·08	......	1·0000	1·0000	1·0000	1
644·59	......	......	1·0041	......	1·0041	$1 + a \div 28$
531·27	531·01	......	1·2182	1·2186	1·2300	$1 + 2a$
509·00	508·83	......	1·2715	1·2717	1·2727	$1 + 5b$
480·56	480·27	......	1·3468	1·3473	1·3450	$1 + 3a$
468·10	......	......	1·3826	......	1·3818	$1 + 7b$
441·94	441·81	......	1·4645	1·4646	1·4600	$1 + 4a$

The quotient of Kirchhoff's sixth wave-length by the seventh ($468·10 \div 441·94$) is equal to the quotient of the fourth by the fifth ($509 \div 480·56 = 1·0592$).

Lanthanum, K.

Wave-lengths.		Quotients.	Theoretical.	
538·56	......	1·0000	1·0000	1
538·43	......	1·0003	1·0003	$1 + \frac{1}{4}a$
538·00	......	1·0011	1·0011	$1 + a$
534·48	......	1·0077	1·0077	$1 + 7a$
520·80	......	1·0341	1·0340	$1 + 31a$
519·20	......	1·0373	1·0373	$1 + 34a$
518·69	......	1·0383	1·0384	$1 + 35a$
481·59	......	1·1183	1·1183	$1 + 108a$

XLII. *The Bearing of the Kinetic Theory of Gravitation on the Phenomena of " Cohesion " and " Chemical Action," together with the important connected Inferences regarding the existence of Stores of Motion in Space.* By S. Tolver Preston[*].

No. IV [†].

1. IT would be natural to expect that any theory competent to explain the effects of gravity ought to be able to throw some light upon the subsidiary effects of molecules exhibited in " cohesion," " chemical action," &c. Before proceeding to consider this question, and in order to have a clear conception of the point we have to deal with, we will recapitulate in a few words the physical conditions involved in the case of gravity as already dealt with. It has been our object to point out that the molecules of a gas *within the range of free path* are moving in precisely the right way to produce

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gravity in two masses immersed in the gas within the range of free path. For since it has been proved from the kinetic theory that the particles of a gas adjust their motions so as to move *uniformly or equally in all directions,* and since the particles within the range of free path are moving in unbroken streams, it follows that two masses immersed in the gas at a distance apart within this range will (owing to the one sheltering the other) be struck with more particles on their remote (unsheltered) sides than on their adjacent (sheltered) sides, so that the two masses will be urged together. This, therefore, fulfils Le Sage's fundamental idea without the necessity for accepting any of his postulates. We need not accept the scarcely realizable postulates of streams of particles coming from indefinite distances in space (at uniform angles), each stream moving continuously in one direction ; but we can substitute for this the natural conception of the normal motion of the particles of a gas within the range of free path, where, although *each* particle is continually changing the direction of its motion, yet the *general character* of the motion of the system as a whole remains unchanged ; or the system of particles automatically correct their motions so as to continue to move *uniformly or equally in all directions,* as demonstrated in connexion with the kinetic theory of gases. This movement of the particles *equally* in all directions is the condition required to produce equal gravific effect in all directions. Thus all we require to admit in order to produce all the effects of gravity as *necessary* results, is the existence of a gas in space. This gas differs from an ordinary gas only as to scale, *i. e.* in the proximity, velocity, and extreme *minuteness* of its particles, whereby a length of free path commensurate with the greatest observed range of gravity is insured, the extreme minuteness of the particles being at the same time adapted to that high velocity which the effects of gravity require, and which also necessarily renders the medium itself impalpable or concealed from the senses. The range of free path, though great in one sense, may be considered small and suitable for a gas that pervades the vast range of the visible universe.

2. In applying these principles to cohesion, or the approach of molecules in chemical reactions, it is so far easy to see that when two molecules of matter come very close together, or if we suppose them actually to come into contact, then they will cut off the *entire* stream of particles of the gravific medium from between the parts in *contact* ; and therefore, as the gravific particles now only strike against the remote sides of the two molecules, the latter will be urged together with very great

force, thus explaining " cohesion "*.   But then a difficulty at
once presents itself here.   When two masses (or molecules)
are gradually approached towards each other, instead of the
tendency to approach gradually increasing up to a maximum
(as we should expect from the theory), they begin to repel at
a certain distance, and very considerable force is in general
required to overcome this first repulsion, when the masses
then unite into one.   Thus two freshly cut pieces of lead may
be made to unite with some pressure, also glass, or various
metals, with more or less pressure.   There is therefore a *neutral
point* which has to be passed, when the tendency to recede
changes into a tendency to approach.   The same thing is ex-
hibited (conversely) when a substance is broken into two
parts by tension.   If pulled (nearly) up to the neutral point,
the two parts recoil or return into their old positions.   If pulled
beyond the neutral point, the parts repel and will not return
into their old positions, *i. e.* they separate permanently.   The
thing, therefore, to be explained is the existence of this *neutral
point*, or, in other words, the repulsion that exists at a certain
distance from the surfaces.

3. The explanation we have to offer here depends upon quite
recent investigations.   It must be observed first that facts
prove the existence of a second medium in space besides the
gravific medium, viz. the heat- or light-conveying medium (the
æther).   If we admit the existence of *one* medium in space
constituted according to the kinetic theory (the gravific
medium), it would be natural to conclude that the second
medium (or æther) was constituted in an analogous manner.
We shall give independent reasons afterwards that lead to
infer this constitution, and endeavour to answer possible objec-
tions ; but in the mean time it is only necessary to suppose it
to be so constituted (in the absence of proof to the contrary) ;
and if this supposition serves to explain in general principle a
number of facts, this will be one argument for its truth.   On
account of the extreme shortness of the waves of light and
heat, it would be reasonable to suppose that the length of free

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* The spectroscope proves molecules to be complex bodies, on account
of the number of different periods of vibration they can take up; and it
was pointed out in the last paper that there are grounds for inferring them
to possess interstices, or a more or less open structure.   It is evident,
therefore, that the *shapes* of molecules, as to whether their parts *fitted*
over each other or not (and thus afforded more or less shelter from the
impinging particles of the gravific medium), would have some influence
on the behaviour of molecules as to the energy of their approach (reac-
tions).   This might account in some degree for the varied behaviour
termed " chemical affinity," though possibly there are, besides this, other
modifying physical conditions.

path of the æther particles was contained within compact limits, or was, at any rate, shorter than the length of the wave itself. It has been proved recently, in investigations by Mr. Johnstone Stoney in connexion with the radiometer[*], that a medium constituted according to the *kinetic* theory has a special power of propagating a pressure *unequal* in various directions, or that, when a layer of the medium (such as a layer of air) is intercepted between two surfaces whose distance apart is a small multiple of *the length of free path* of the particles of air, the layer can then transmit a pressure in the line perpendicular to the surfaces which is *in excess of* the transverse pressure ; and thus a repulsion is produced, accounting for the spheroidal state, the motion of the radiometer, &c. In fact it is evident (as pointed out) that, since in a medium constituted according to the kinetic theory the particles move in straight lines, the particles (when the distance of the opposed surfaces approximates to the range of free path) get reflected backwards and forwards repeatedly between the opposed surfaces, the increments of energy received by the particles accumulating by successive reflections, so that the particles produce a bombardment tending to separate the two opposed surfaces [†]. The increments of velocity imparted by the heated

[*] Philosophical Magazine, December 1877.

[†] There is another point in connexion with the motion of the particles, which no doubt, however, has been already noticed. Under normal conditions, a body vibrating opposite to another tends (as is known) to produce rarefaction in the intervening medium; but in the case of a film whose thickness is near the range of *mean path* of the particles, there would appear to be a special cause tending greatly to reduce this effect, and even perhaps to produce the contrary effect, viz. a condensation (which would greatly increase the repulsion). Thus, under the increments of velocity received, there is a tendency for the molecules of the gaseous film to be turned round so as to move more *normal* to the film. Suppose, for instance, an elastic sphere to be rebounding *obliquely* between two planes. Suppose increments of velocity to be given to the sphere by vibrating one of the planes. Then these increments of velocity given to the sphere will evidently make it rebound more *normal* to the surfaces. So in the case of the molecules of a gaseous film, rebounding backwards and forwards between two surfaces (such as the air film which supports a drop in the so-called " spheroidal " state, the air film which supports a grain of powder in some experiments of Professor Barrett, referred to by Mr. Johnstone Stoney), the molecules of the film will tend, by the increments of velocity given them, to turn round so as to move in a direction more normal to the surfaces. This evidently makes the lateral pressure exerted by the film less, and consequently its lateral expansion (or rarefaction) less. If we imagine the extreme case where the molecules of the film are all turned round so as to move *exactly* normal to the surface of the film, then whatever the velocities of the molecules of the film (*i. e.* whatever the *longitudinal* pressure, or repulsion, exerted by them), the film would exert no *lateral* pressure at all. There would consequently be a lateral inrush of air, increasing the *density* of the film, and therefore in-

surfaces are also mainly received in the line joining the surfaces (not so much transversely); so that this conduces to the pressure on the surfaces, or *repulsion*.

4. This is precisely what we have to put forward, *in its application to the æther*, as an explanation of the repulsion in the cases referred to, such as for example the repulsion of two lenses or glass surfaces placed together in such proximity as to exhibit " Newton's rings," the repulsion of two molecules &c.; for if the æther be constituted according to the kinetic theory, we shall inevitably have the same phenomena here, though on an infinitely more energetic scale ; for the particles of æther come into direct contact with the vibrating molecules of matter, whose energy of vibration is known to be enormous at normal temperature ; and the layer of æther is very thin, and the motion of the æther particles very rapid *, so that the successive increments of velocity imparted by the vibrating molecules accumulate by successive reflections (backwards and forwards) between the opposed surfaces, producing a forcible repulsion. These results have been theoretically demonstrated to follow on the basis of the *kinetic* theory, and have been established by experimental facts. It is a point of great importance to observe that it is specially the *kinetic* theory that explains this otherwise most curious fact of an *excess* of pressure in a medium in one direction (producing a repulsion), with *normal* pressure existing in transverse directions, which otherwise it would be so difficult to explain, and which must be explained in order to account in a realizable manner for the phenomena observed. It is difficult to conceive how any other means of explaining this curious fact could be afforded than that supplied by the kinetic theory. Moreover it is generally admitted that *heat* has the property of producing repulsion. The " heat " of the molecules in the cases mentioned is known to consist in their *vibrations*, by which they generate *waves* of heat in the æther. We have therefore to explain under what particular constitution of a medium *vibrations* can (within certain limits) produce repulsion. The *kinetic* theory of the constitution of the medium solves completely this peculiarly difficult problem.

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creasing the repulsion (since those molecules which enter the film become themselves available for producing repulsion). Possibly, from this cause, these films may be actually *denser* than normal density. At all events the above cause makes their density greater than it otherwise would be, and the repulsion exerted by them greater.

* It may be noted that, *if* the æther be constituted according to the *kinetic* theory, the normal velocity of its particles is $\dfrac{3}{\sqrt{5}} \times$ velocity of a wave of light. See appendix to paper " On the Mode of Propagation of Sound " (Phil. Mag. June 1877), added by Prof. Maxwell.

5. When the two surfaces (or two molecules) are pushed up closer to each other, then the energy of the gravific medium directed against the remote sides of the molecules prevails more and more, since the mutual sheltering-power of the molecules increases in an enormously rapid ratio as contact is neared, and so the unbalanced energy of the gravific medium directed with full force against the remote sides of the opposed molecules at length outweighs the action of the intercepted æther particles, and the two molecules are propelled together (or unite).

6. We may allude to a few examples serving to illustrate the application of the above principles. Supposing we take the common case of the ignition of a gas jet. Then when the gas is turned on, the molecules of gas and air mingle with each other and are known to be exchanging motion and rebounding from each other, and yet they do not unite. According to the above principles the molecules, as they approach each other in their encounters, are kept apart by the forcible vibrations (which the molecules are known to possess *) which, through the increments of velocity imparted to the particles of the intervening æther, produce a repulsion in the manner described, as soon as the molecules in their encounters have approached nearly within range of the *mean path* of the æther particles. When a flame is applied to the jet, the rapidly moving gaseous molecules of which the flame consists naturally produce a disturbance, jostling some of the molecules of the mixture of gas and air against each other, so that the neutral point is passed, whereby the molecules are brought into such proximity that their mutual sheltering action causes the gravific medium to impinge with full energy upon their remote sides, thus urging the molecules together (producing combination). The molecules are thrown into forcible vibration by the shock of approach, and become luminous through the energy of the waves thus generated by them in the surrounding æther. These vibrations of the compound molecules after combination naturally cause the forcible rebound of any other molecules that happen to be in their proximity, the disturbance thus set up sufficing to effect the successive (practically instantaneous) combination of the entire jet of gas. The same considerations of course apply to the practically instantaneous combination (explosion) of a mixture (in definite proportions) of gas and air, by an initial disturbance

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* The molecules of matter in the gaseous state are known to possess, *in addition* to the translatory motion peculiar to that state, a vibratory motion, in virtue of which the molecules generate waves of regular periods in the æther (these periods having in many cases been measured by the spectroscope).

produced by a flame. In the case of solid bodies, where the molecules are fixed or under control, a forcible pressure or concussion may serve to bring the molecules over the neutral point (and thus effect combination), as illustrated by the effect of the blow struck in " percussion " powders. It would not appear that matter in the gaseous state could ever be exploded by pressure (so long as the gaseous state was retained) ; for the molecules of gases cannot be pressed against each other by any amount of pressure, since, the molecules being in free trans-latory motion among themselves, the only effect of pressure would evidently be to put a greater number of molecules into unit of volume, without thereby causing the molecules in their encounters to approach nearer to each other than before. The degree of approach of the molecules (in their encounters) depends evidently on their momentum or velocity ; and this remains the same whatever the pressure.

7. Heat could not apparently be said to augment the energy of chemical combination, since, in general, heat is known to possess the exactly opposite effect, or to disintegrate matter. The part played by heat in effecting chemical combination would seem to consist simply in producing a molecular *dis-turbance*, whereby unavoidably some molecules are urged towards each other so as to pass the outer neutral point, which is the necessary preliminary to combination. No doubt, when heated elements combine, the original heat adds itself to the work thus to be derived, as the heat cannot be destroyed, though it cannot increase the work of combination. Heat may (as is known) entirely prevent chemical combination, and even dissociate combined elements. The action of heat in preventing chemical combination and producing dissociation would on the above principles consist in the fact that, when the vibratory motion of the molecules becomes excessive, this vibratory motion generates such a pressure in the intervening layer of æther on the approach of the molecules as to prevent them from passing the neutral point : or, indeed, no neutral point may exist, provided the pressure or repulsion thus generated be such as to outweigh the action of the gravific medium, as appears actually to take place in the dissociation of matter by excessive heat. Thus it would appear probable from this, that when combination ensues in the case of a mix-ture of gases previously considerably heated (but not so much so as to produce dissociation), the molecules on combination do not at once settle down into that full proximity (which belongs to a lower temperature), but they do so gradually as the temperature falls. Thus the work of combination is pro-longed over the falling temperature, and the cooling thereby

somewhat retarded.   Precisely the same thing is illustrated in the aggregation of groups of molecules (to form masses), as in the aggregation of single molecules to form compound molecules.   Thus when a bar of iron is welded by heat, the molecules (though aggregated or combined) do not settle down into their final positions of proximity until the bar cools, the bar being observed to contract on cooling.   In this instance also the cooling of the bar is somewhat retarded by the approach of the molecules in the act of cooling.

8. In the case of the ignition of a solid body, the same considerations no doubt apply as in the case of a gas.   Thus, for example, the molecules of oxygen are impinging against the surface of a piece of coal, but do not produce ignition.   To effect this a certain number of the molecules must be impelled with sufficient energy against the coal so as to carry them over the neutral point (*i. e.* beyond the initial repulsion).   The application of a flame, which consists of matter in a state of violent agitation, suffices to effect this, and, no doubt by loosening some of the molecules of carbon (of the coal) and giving them translatory motion and mixing them with the air, facilitates the process.

9. As a further illustration of the exact similarity of behaviour of *single* molecules and groups of molecules (masses) as regards the existence of the above-mentioned neutral point, we may take the case of the substance iodine.   This substance gives off a visible vapour at normal temperatures.   The single molecules of iodine composing the vapour rebound from each other without uniting; and this can only be due to the existence of the above-mentioned neutral point, outside which there is a repulsion.   If the colliding molecules were to approach within the neutral point, they would unite and form solid iodine.   No doubt *some* of the molecules of the vapour (as their velocities are known to be very diverse) do pass beyond the neutral point; and thus molecules of vapour striking against the fragments of solid iodine in the bottle, will sometimes unite with the solid iodine and form part of it, while, on the other hand, other molecules of the solid which happen to possess excessive vibrating energy are thrown off, this being the known way in which the balance in evaporation is maintained. The masses of iodine have the same neutral point as the single molecules, since two masses of the substance when pressed together will not readily unite; *i. e.* the neutral point, where the outer repulsion terminates, must be passed first[*].

* The above effects were described in a little book ' Physics of the Ether' (E. & F. N. Spon), published by me in 1875; but the *cause* of the reduction of the pressure of the medium, which determines the approach

10. Just as increase of vibrating energy (temperature) tends, by the increase of pressure thus produced in the intervening film of the medium, to dissociate molecules, so reduction of vibrating energy (attendant on reduction of temperature) tends to facilitate the approach of molecules, on account of the reduction of the pressure or repulsive action of the intervening film. Thus the molecules of a vapour when their vibrating energy is reduced (by a fall of temperature) may by the simple momentum of their own encounters, carry themselves over the neutral point, and thus effect the condensation of the vapour. Numerous other cases might be cited illustrative of the application of the above principles, as, indeed, the molecular effects are very similar in their fundamental aspects. The molecular phenomena, however diverse, may be all correlated in one fundamental respect, viz. as consisting in phenomena of *approach* and *recession*. The fundamental conditions to be explained, therefore, are the conditions capable of producing the *approach* and *recession* of molecules. Whatever may be said of the above deductions, it is at least so far certain that the conditions investigated, and based upon experimental facts, are competent to produce these fundamental movements of approach and recession in the case of molecules, and to do so in the *simplest* manner, the constitution of media according to the kinetic theory being admittedly the *simplest* conceivable. To look therefore to other conditions than the simplest would be to imply that the same results are brought about by a superfluity of mechanism. This superfluity is known *not* to be the characteristic of nature ; and all the teaching of mechanism points to the fact that superfluity or unnecessary complication entirely prevents the attainment of precision and certainty in the mechanical effects. The great precision and unfailing certainty of the molecular effects would therefore render it *necessary* to infer that the regulating mechanism was simple, or that there was no unnecessary superfluity.

11. The fundamental conclusion above drawn regarding the mechanism concerned in the *approach* of molecules is grounded upon the only explanation of the mechanism of gravity that has withstood criticism and received support by competent judges, viz. the kinetic theory of gravity, of which Le Sage's ingenious idea forms the fundamental basis, and is at once the *simplest* explanation of gravity conceivable. The

of molecules, was there wrongly stated, the error having arisen from a seeming analogy between the approach of bodies to masses (tuning-forks &c.) vibrating in air—in the absence of the knowledge recently acquired of the repulsion of gaseous layers. Much of the main principles of the book, however, remain as they were—to be supplemented by the investigations contained in the present papers.

application of this theory to molecules in close contact (" cohesion " &c.), is *necessary* and inevitable, and it serves to correlate the molecular effects generally under *one* cause. The explanation of the fundamental condition capable of producing the *recession* of molecules, as above given, rests upon experimental facts recently established, and upon a basis for the constitution of the æther which is the *simplest* conceivable.

12. We now propose to show some *independent* reasons in support of this constitution for the æther, in addition to the argument afforded by the numerous molecular effects which this constitution, in principle, serves to explain. First, if the subject be reflected on, it will be apparent that, in principle, a movement of the component particles of the medium *in straight lines* is the only possible constitution for the *ultimate* medium in space. For a particle of matter cannot move in a curved line unless it have a medium about it to control its motion. Thus a planet can move in a curve because it has a medium about it (the gravific medium) to cause it to move in a curve. It is a known principle *that a particle of matter cannot of itself change the direction of its motion.* The particles of the *ultimate* medium in space must therefore move *in straight lines*. This deduction is surely of great importance in the inquiry as to the constitution of the physical media in space. Also in addition to this, the observed facts of gravity prove that the particles of the *gravific* medium move in straight lines, since no other motion than this can harmonize with the observed effects of gravity. It would therefore surely be a strange thing if the particles of the æther, as a second medium immersed in the gravific medium, did not move in straight lines. To suppose this would be very like supposing that when the particles of a second gas are immersed among those of another, the particles of the first gas acted upon those of the second to make them move otherwise than in straight lines, which is known to be impossible. Moreover the fact of the kinetic theory representing the *simplest* conceivable constitution for a medium would by itself be a strong argument for this constitution in the case of the æther. The very fact of the great precision and delicacy of the operations performed by the æther as the mechanism for the transmission of the varied phenomena of colour &c. would point to a simple constitution ; just as the complex effects of sound with all its intricate and varied gradations of tone are known to be transmitted by a medium (the air) of the *simplest* conceivable constitution, viz. that represented by the beautiful kinetic theory of gases*. The more intricate the functions of a mechanism,

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* There would surely be nothing to admire in complication in itself. The whole aim of mechanical design is directed towards the attainment of *simplicity*, which being *unique*, entails intellectual labour to find it.

the more is simplicity indispensable, and superfluity incompatible with precision and certainty in the results.   To assume a constitution for the æther that could not be realized or clearly explained would surely be futile, since the explanation or clear conception forms the logical support of any theory, without which the theory resembles a mere dogmatic statement incapable of being sustained by reason.

13. There is one other point which we would notice in connexion with this subject.   The idea would appear to be to a certain extent prevalent that the æther must have a constitution essentially different from the air, because the vibrations producing light are transverse, while those producing sound are longitudinal.   It seems to be sometimes inferred from this that the vibrations of the æther are *only* transverse, and those of the air *only* longitudinal.   There would be no warrant for this conclusion ; and we think that it has done harm and greatly hindered any rational idea from being formed of the nature of the æther.   According to the kinetic theory, which is known to represent the constitution of the air, the vibrations of the particles of air disturbed by a vibrating body and propagated in the form of waves, are not *only* longitudinal ; for since according to the kinetic theory the particles of air in their normal state are moving equally in *all* directions, it follows that these particles are accelerated and retarded both in *transverse* and in longitudinal directions at the passage of waves.   It is true that the *transverse* component of the motion probably may not affect the ear, on account of its special structure.   It would be wrong, however, to infer from this that the *transverse* component of the motion did not exist. So in the case of the æther, it would be unwarranted to infer that the longitudinal component of the motion did not exist, because this component was incapable of affecting the eye. The eye and the ear may be very differently constituted ; and a motion that affects the one might not affect the other.   Sir John Herschel says regarding this point in his essay " On Light " ('Popular Lectures on Scientific Subjects,' page 358) :—" According to any conception we can form of an elastic medium, its particles must be conceived free to move (within certain limits greater or less according to the coercive forces which restrain them) in every direction."   He then goes on to explain how the efficacy of the transverse component of the movement in the case of light, and the longitudinal component of the movement in the case of sound, may be accounted for by the diverse structure of the eye and ear.   Any inference which is not valid, invariably does some harm ; and this idea of a forward movement being propagated in a medium by *only* transverse vibrations, being almost inconceivable, has

naturally led to some incongruous ideas regarding the structure of the æther, in the effort to explain it. Thus some have supposed the æther to resemble a solid, which is in direct opposition to the teaching of the senses ; for we move about so freely in this "solid" as to be unconscious of its existence. Another supposition has been that "lines of tension," behaving somewhat in analogy to stretched chords, exist in the æther. Such a mechanism would be, to say the least, somewhat deranged by the passage of a planet through the æther. Indeed it is sufficiently evident that these are the hopeless attempts made to surmount an impossible condition, or a difficulty for whose existence there is really no warrant. If the æther be not a solid, or a liquid (for liquids oppose enormous resistances to the passage of bodies through them at high speeds), then what other resource have we than to conclude that it is a gas ?

14. A gaseous constitution of the æther according to the kinetic theory would perfectly satisfy the two fundamental conditions of a medium highly elastic in all directions, and opposing no appreciable resistance to the free movement of bodies (the planets &c.) through its substance. For it is a known fact that the resistance opposed by a medium constituted according to the kinetic theory to the passage of bodies through it *diminishes* as the normal velocity of the particles of the medium increases. The high normal velocity of the particles of the æther, proved by the velocity of light, therefore necessarily renders the resistance inappreciable, and the medium itself impalpable and undetected by the senses.

15. A difficulty has been raised in the way of the æther being constituted as a gas on the following grounds, which, being only anxious for truth, we are bound to consider *. It has been argued that if the æther be constituted as a gas, the specific heat of unit of volume of the æther would be the same as that of any ordinary gas at the same pressure, and that therefore it would appear that the presence of the æther could not fail to be detected in the experiments on the specific heat of ordinary gases. We have to offer the following as a means of meeting this difficulty. It will be admitted that the detection of the æther in the experiments on specific heat will depend, *not* on the specific capacity for heat possessed by the æther, but on the *rate* at which the heat passes from the gas experimented on to the æther. The molecules of the gas are moving through the æther with their normal translatory motion, this motion of the molecules representing the "heat" of the gas. It will be evident that the rate at which the

* See paper "On the Dynamical Evidence of the Molecular Constitution of Bodies," by Prof. Maxwell ('Nature,' March 11, 1875).

motion ("heat") of the molecules of the gas passes to the æther will depend on the *resistance* the æther offers to the passage of these molecules through it. But we have shown that this resistance may (on account of the high normal velocity of the æther particles) be inappreciable. Hence the rate of passage of the heat from the gas to the æther will be inappreciable. This, we submit, removes the difficulty in question. It is clear that, if the æther opposes no appreciable resistance to the passage of a planet through it (moving at several miles per second), it cannot be affected by the passage of a molecule of a gas through it, which in its relatively slow rate of translatory motion may be considered *at rest* compared with the æther particles. The high normal velocity of the æther particles is only appropriate to their *minute* mass.

16. It must be apparent to any reflecting observer, that in physical science we have a vast array of *facts* accumulated through years of experiment, but a great paucity of *causes*; or the number of facts known is quite out of all proportion to the number of causes known, these latter being replaced by more or less vague and unsubstantial theories. As, therefore, we have no paucity of facts as a basis to reason upon, it surely cannot be too soon to make an effort to correct this anomalous state of things, and to replace the above unsubstantial theories by rational conceptions of the processes of nature. Clearness of conception is the test of truth, and constitutes its real dignity; and theories, however elaborated, if vague, have no real dignity[*]. Since there is nothing occult about the physical media in space, in so far as they differ in no way from ordinary matter excepting in the mere scale or dimensions of their parts, and since it is obviously just as easy to reason of matter of one dimension as of another, any hesitation in entering upon this course of study would be wholly uncalled for; indeed, surely there is reason for a rational interest in realizing the admirable adaptation of these media in a mechanical point of view for their special functions; and the question as to the utilization of the stores of motion enclosed by them to the best advantage may present a problem of the highest practical interest and importance [†]. It should be observed that these stores of

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[*] Vagueness, paradox, and mystery surely belong rather to those intellects which are incapable of rising to clear and definite conceptions.

[†] As an instance of the change of views on the most practical subjects that the acceptance of these principles entails, we may cite the case of the employment of coal, which by the recognition of the existence of the stores of motion in space, becomes a *mechanism* or machine for deriving motion. The expenditure of coal, therefore, represents the expenditure of mechanism or *machinery*. Hence in deriving motion through coal we expend a quantity of *machinery* proportional to the power derived. Without asking the question whether it is necessary in every case, in deriving motion from a source, to expend machinery proportional to the

motion simply consist in small particles of matter in a state of rapid motion; or there is nothing occult about the subject at all, as indeed obviously principles of reasoning are independent of *size*. The minute size (and consequent invisibility) of the particles is necessary to the *efficiency* of the media as powerful motive agents, since minuteness of size is necessary to render a high velocity possible for the particles, without producing disturbing effects among the matter immersed in these media. There is one very noteworthy point that cannot be too distinctly kept in view in connexion with this subject. It is the fact that the high intensity of the stores of motion possessed by these media, and which renders them so important, serves to conceal their existence from the senses. Thus the higher the intensity of the store of motion enclosed by these media, and consequently the greater their capacity for practical utility, the more likely (if the mere evidence of the senses were relied on) is their existence to be forgotten. For it may be *proved* beforehand, by the kinetic theory of gases, that the greater the velocity of the component particles of a medium, and consequently *the greater the value of the store of energy enclosed* (which may even reach an explosive intensity), the more does the presence of the medium elude detection, because the resistance opposed by the medium to the passage of bodies through it *diminishes* as the velocity of the particles increases. The less indication the mere senses (unaided by reason) afford of the existence of such media, the higher, therefore, should we be warranted in inferring their importance to be. Even independently of all question of the existence of these media, it may be *proved* beforehand that, *if* media did exist and enclose stores of motion to an enormous intensity, *they would be concealed.* This is, no doubt, a remarkable fact, and contrary to preconceived ideas, as it would doubtless appear on the first thought that the *higher* the intensity of a store of energy existing in space, the *more* likely would it be to make itself apparent to the senses, whereas precisely the contrary is found to be the fact. This forms a notable instance of one of those cases where analysis completely reverses preconceived ideas. It is possibly the absence of appreciation of this fact that may in some way account for the failure of the most striking proofs of nature to carry their practical teaching, as for example, the sudden setting free of concealed motion in the explosion of a mass of gunpowder. Here to the mere bodily senses, we have apparently an actual *creation of motion.* Something

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power derived (*i. e.* that the work done should be the equivalent of the *machinery* expended), it is at least so far certain that no remedy for this could be discovered unless the physical conditions of the case were recognized.

more, however, than the evidence of the mere bodily senses
may be required, to appreciate the truths of nature, as it is a
notorious fact that the most important truths generally lie
below the surface. It should be noted that these media would
not be *efficient* as working agents unless they were concealed ;
for concealment (as observed) is the *necessary* condition to the
enclosure of a store of motion to a high intensity. Possibly
the absence of realization of this fact, and perhaps that pre-
judice which besets every new path, may in some degree
account for what must otherwise appear an extraordinary
indifference and absence of inquiry in a subject of great me-
chanical interest and involving possibly issues of the highest
importance and practical utility. When this, like every other
illogical prejudice to change, comes to be broken down by the
light of reason and reflection, there may be just ground for
surprise at the previous delay, and at the shallow and unsub-
stantial character of the theories which so long supplanted
rational conceptions of the processes of nature.

London, March 13, 1878.

XLIII. *Notices respecting New Books.*

*Des Paratonnerres à Pointes à Conducteurs et à Raccordements Ter-
restres Multiples. Description détaillée des Paratonnerres établis sur
l'Hotel de Ville de Bruxelles en* 1865. *Exposé des motifs des dis-
positions adoptées par* MELSENS, *Membre de l'Académie Royale des
Sciences de Belgique.* Bruxelles : Hayez. 1877.

THE work, of which the title is given above, possesses an interest
not only to the electrician and man of science, but to the
architect and antiquary; and Professor Melsens, already so favour-
ably known to the world of science, has done further good service
in applying sound scientific principles to the preservation of those
grand monuments of mediæval architecture of which his country is
so justly proud. The origin of the work which we now notice was
in the circumstance that, in a thunder-storm in 1863, one corner
tower of the Hotel de Ville at Bruxelles was struck by lightning,
and that portion of the building seriously injured, whilst the very
much higher central tower and spire were not touched. The muni-
cipal authorities of Bruxelles at once proceeded to consider the steps
to be taken to preserve the building from any future like injury,
and requested the assistance of the Academy of Sciences, which
appointed a committee. Opinions were very much divided, and no
practical conclusion was arrived at; so the Municipal Council
very sensibly passed over the Committee and placed the whole
matter in the hands of Professor Melsens, who had formed definite
ideas on the subject, and was prepared to take the responsibility of
carrying them out. The great difference of opinion among scientific
men was as to the comparative advantages of concentrating the
means of defence in one or a very few lightning-conductors ex-
tended to a great height, overtopping the most elevated portions of

the building, but leaving the lower portions and general mass of the building unprotected, or (2) of supplying all, even the lower portions of the edifice, with conductors of lesser height and dimensions, but all connected among themselves, and all leading finally to some great conducting mass of earth or water into which the electric discharge should finally find issue. M. Melsens was led to conclude decidedly in favour of the latter plan, and to adopt in electricity, as is often done in public matters, the maxim *divide et impera*. He was struck with the fact that, although dating from 1400, the Hotel de Ville had never been struck by lightning until the present century, and has since been struck several times, although seriously damaged only on the last occasion, in 1863. He attributes this remarkable immunity of four centuries to the amount of gilding and metallic decoration &c., which almost covered over the surface of the building in the later middle ages and down to the end of the last century—the projecting pinnacles and statues especially having been then blazing with gold and brass, thus furnishing an enormously extended, if superficial, issue for the electric discharge. On the other hand, in the case of the last thunder-storm he finds the greatest injury to have occurred where parts of the towers and pinnacles were supported by iron bars which were unconnected with each other or with the ground. Where perfect connexion existed, even, as in the case of the great clock, by a few thin wires, the electric discharge passed harmlessly along; and the fortunate escape of the great central spire from destruction is thus to be accounted for.

Acting on those principles, M. Melsens devised an arrangement for investing the Hotel de Ville with a connected series of conductors moderate in mass and in height, terminating on each projecting eminence of the building in pencils of wire, but not prominently interfering with the architectural characteristics. In this way the edifice is surrounded by a complete cage of iron wire, so that a flash of lightning striking on any point must be immediately subdivided into a multitude of parts and so diluted as to be rendered innocuous. This iron cage is in its turn connected with what M. Melsens terms the subterraneous paratonnerres, consisting of the whole system of pipes employed in the gas- and water-distribution of the city; and, from the details of the mechanical arrangements given, this connexion is of the most perfect kind. On this point M. Melsens lays considerable stress, as he believes that very many lightning-conductors of the ordinary kind are rendered useless practically by the imperfect connexion with a sufficient mass of earth and water conductor. We must refer to the work itself for all the mechanical details by which M. Melsens's ideas have been practically carried out, and which are copiously illustrated by engravings. We shall not either enter into the discussion as to the merits of the questions still at issue among practical electricians of great eminence and experience. M. Melsens gives in his book a full and fairly stated *résumé* of the arguments and evidence against as well as in favour of his own views; and that his views, even when deviating from the traditional decisions of scientific authorities (as, for example, of

the Paris Academy of Sciences), have been approved and acted upon by competent judges, is shown by the fact that the charge of preserving from further electric injury the grandest architectural monument of Belgium has been intrusted to his care.

We recommend Professor Melsens's work to the careful perusal of all who are interested in the preservation from injury by lightning of ships and buildings, as well for its useful mechanical suggestions as for its interesting and accurate scientific details.

XLIV. *Proceedings of Learned Societies.*

GEOLOGICAL SOCIETY.

[Continued from p. 237.]

January 23, 1878.—Prof. P. Martin Duncan, M.B., F.R.S., President, in the Chair.

THE following communication was read :—

1. " On the Secondary Rocks of Scotland.—Part III. The Strata of the Western Coast and Islands." By John W. Judd, Esq., F.R.S., F.G.S., Professor of Geology in the Royal School of Mines.

The existence of scattered patches of fossiliferous strata lying between the old gneissic rocks and the masses of Tertiary lava in the Hebrides, has been known to geologists for more than a century. By Dr. Macculloch, who did so much for the elucidation of the interesting district in which they occur, these strata were referred to the Lias ; but Sir Roderick Murchison showed that several members of the Oolitic series were also represented among them. Later researches have added much to our knowledge of the more accessible of these isolated patches of Jurassic rocks in the Western Highlands.

During the seven years in which he has been engaged in the study of these interesting deposits, the author of the present memoir has been able to prove that not only is the Jurassic system very completely represented in the Western Highlands, but associated with it are other deposits representing the Carboniferous, Poikilitic (Permian and Trias), and Cretaceous deposits, the existence of which in this area had not hitherto been suspected ; and by piecing together all the fragments of evidence, he is enabled to show that they belong to a great series of formations, of which the total maximum thickness could have been little, if any thing, short of a mile.

The relations of the scattered patches of Mesozoic strata to the older and newer formations respectively, are of the most interesting and often startling character. Sometimes the secondary rocks are found to have been let down by faults, which have placed them thousands of feet below their original situations, in the midst of more ancient masses of much harder character. More usually they are found to be buried under many hundreds, or even thousands of feet of Tertiary lavas, or are seen to have been caught up and enclosed between great intrusive rock-masses belonging to the same period as the superincumbent volcanic rocks. Occasionally the only evidence which can be obtained concerning them is derived from fragments originally torn from the sides of Tertiary volcanic vents, and now found buried in the ruined cinder cones which mark the

sites of those vents. In some cases the mineral characters of the strata have been greatly altered, while their fossils have been occasionally wholly obliterated by the action of these same igneous forces during Tertiary times.

In every case the survival to the present day of the patches of Secondary rocks can be shown to be due to a combination of most remarkable accidents; and a study of the distribution of the fragments shows that the formations to which they belong originally covered an area having a length of 120 miles from N. to S., and a breadth of 50 miles from E. to W. But it is impossible to doubt the former continuity of these secondary deposits of the Hebrides with those of Sutherland to the north-east, with those of Antrim to the south, and with those of England to the south-east. From the present positions of the isolated fragments of the Mesozoic rocks, and after a careful study of the causes to which they have owed their escape from total removal by denudation, the author concludes that the greater portion of the British islands must have once been covered with thousands of feet of secondary deposits. Hence it appears that an enormous amount of denudation has gone on in the Highlands during Tertiary times, and that the present features of the area must have been, speaking geologically, of comparatively recent production—most of them, indeed, appearing to be referable to the Pliocene Epoch.

The alternation of estuarine with marine conditions, which had, on a former occasion, been proved to constitute so marked a feature in the Jurassic deposits of the Eastern Highlands is now shown to be almost equally striking in the Western area; and it is moreover pointed out that the same evidence of the proximity of an old shore-line is exhibited by the series of Cretaceous strata in the West.

The succession and relations to one another of the series of deposits, now described as occurring in the Western Highlands, is given in the following Table:—

Miocene Volcanic and Intervolcanic Rocks.

	UNCONFORMITY.	
		Max. thicknesses. feet.
Cretaceous.	1. Estuarine clays and sands with coal	20+
	2. White Chalk with flints (Zone of *Belemnitella mucronata*)	10+
	3. Estuarine Sandstones with coal	100
	4. Upper Greensand beds	60
	UNCONFORMITY.	
Jurassic.	5. Oxford clay...	?
	6. Great Estuarine Series	1000
	7. Lower Oolite	400
	8. Upper Lias..	100
	9. Middle Lias	500
	10. Lower Lias	400
	11. Infralias	200
	12. Poikilitic..	1000+
	UNCONFORMITY?	
	Carboniferous strata (Coal-measures).	
	UNCONFORMITY.	
	Old Gneiss Series and Torridon Sandstones.	

Although no traces of the Upper Oolite or the Neocomian formations have as yet been detected in the Western Highlands, yet it is argued that when we consider how enormous has been the amount of denudation, and how singular the accidents to which all the existing relics of the Secondary period have owed their escape from total destruction, we cannot but regard it as a most rash and unwarrantable inference to conclude that no deposits belonging to those periods were ever accumulated within the district under consideration.

The Carboniferous strata of the Western Highlands have been detected at but a single locality, and even there, being exposed in a series of shore-reefs that are only occasionally well displayed, can only be studied under favourable conditions of tide and wind. They consist of sandstones and shales with thin coaly seams ; and their age is placed beyond question by the discovery in them of many well-known plants of the coal-measures, including species of *Lepidodendron*, *Calamites*, *Sigillaria*, and *Stigmaria*.

The Poikilitic strata consist of conglomerates and breccias at the base, graduating upwards into red marls and variegated sandstone, which contain concretionary limestones and occasional bands of gypsum. These strata have not as yet, like their equivalents in the Eastern Highlands (the Reptiliferous Sandstone of Elgin and the Stotfield rock), yielded any vertebrate remains. They were evidently deposited under similar conditions with the beds of the same age in England, and are not improbably of lacustrine origin.

The Jurassic series presents many features of very great interest. The Infralias is better developed than is perhaps the case in any part of the British Islands ; and in the district of Applecross a series of estuarine beds containing thin coal-seams is found to be intercalated with the marine strata.

The Lower Lias, in its southern exposures, presents the most striking agreement with the equivalent strata in England, but when traced northwards exhibits evidence of having been deposited under more littoral conditions : the lower division (Lias α, Quenstedt) is represented by a great thickness of strata ; while the upper (Lias β) is absent or rudimentary. The Middle Lias is grandly developed, and consists of a lower argillaceous member and an upper arenaceous one, the united thickness of which is not less than 500 feet. The Upper Lias singularly resembles, in the succession of its beds and its palæontological characters, the same formation in England. The Inferior Oolite is formed by series of strata varying greatly in character within short distances, and betraying sufficient signs of having been accumulated under shallow-water conditions. Above the Inferior Oolite we find a grand series of estuarine strata, partly arenaceous and partly calcareo-argillaceous ; and this is in turn covered conformably by an unknown thickness of blue clays with marine fossils of Middle Oxfordian age. At the very lowest estimate, the Jurassic series of the Western Highlands could not have had a thickness of less than 3000 feet !

The Cretaceous strata of the Western Highlands, though of no great thickness, are of surpassing interest. They consist of two

marine series alternating with two others of estuarine origin. At the base we find marine deposits of Upper Greensand age, strikingly similar to those of Antrim, but in places passing into conglomerates along old shore-lines. Above the Upper Greensand beds occur unfossiliferous sandstones, in which thin coal-seams have been detected; and these are in turn covered by strata of chalk, converted into a siliceous rock, but still retaining in its casts of fossils (*Belemnitella*, *Inoceramus*, *Spondylus*, &c.), and in its beautifully preserved microscopic organisms (Foraminifera, *Xanthidia*, &c.) unmistakable proofs of its age and the conditions of its deposition. Above this representative of the highest member of the English Chalk there occur argillaceous strata with coal seams and plant-remains which are perhaps the equivalent of younger members of the Cretaceous series, not elsewhere found in our islands; or, it may be, they must be regarded as belonging to periods intermediate between the Cretaceous and Tertiary epochs. It is greatly to be regretted that these Cretaceous deposits of the Western Highlands are so unfavourably displayed for our study as to present scarcely any facilities for the collection of their fossils; for these, if found, might be expected to throw a flood of light on some of the most obscure palæontological problems of the present day.

Although the comparison and correlation of the Secondary strata of the Highlands with those of other areas, and the discussion of the questions of ancient Physical Geography thereby suggested, are reserved for the fourth and concluding part of his memoir, the author takes the opportunity of making reference, in bringing the present section of his work to a close, to several problems on which the phenomena now described appear to throw important light. In opposition to a recent speculation, which would bring into actual continuity the present bed of the Atlantic and the old Chalk strata of our island, he points to the estuarine strata of the Hebrides as demonstrating the presence of land in that area during the Cretaceous epoch. He also remarks on the singular agreement of the conditions of deposition of both the Silurian and Cretaceous strata of the Scottish Highlands and those of the North-American continent. But he more especially insists on the proofs which we now have that the Highlands of Scotland, as well as the greater part of the remainder of the British Islands, were once covered by great deposits of Secondary strata, and that the area has been subjected to enormous and oft-repeated denudation. He dwells on the evidence of the vast quantities of material which have been removed subsequently to the Mesozoic and even to the Miocene period; and he maintains the conclusion that many, if not all, of the great surface-features of the Highlands must have been produced during the very latest division of the Tertiary epoch, namely the Pliocene.

XLV. *Intelligence and Miscellaneous Articles.*

ON GALVANIC CURRENTS BETWEEN SOLUTIONS OF DIFFERENT
DEGREES OF CONCENTRATION OF THE SAME SUBSTANCE, AND
THEIR SERIES OF TENSIONS. BY DR. JAMES MOSER.

THE electromotive force of liquid galvanic series is influenced by the concentration of the liquids. In order to determine the nature of this influence, I have investigated, in the laboratory of Professor Helmholtz, liquids with which it is possible to isolate this influence of the concentration. All chemical processes were to be excluded; therefore only differences of concentration might exist or changes in it occur during the passage of the current. Likewise, for the elimination of all chemical actions, it was necessary that the electrodes should consist of that metal which was contained in the solution.

Two glasses with differently concentrated solutions of the same salt were connected by a siphon; and the circuit was closed by a metallic conduction with the electrodes just mentioned. I then observed, in all the cases investigated, *that a current arises which proceeds from the more dilute to the more concentrated solution.* It may be represented thus :—

$$\text{Zn,} \quad \text{dilute Zn SO}_4, \quad \text{concentrated ZnSO}_4, \quad \text{Zn.}$$
$$\longrightarrow$$

This current appeared regularly in a series of solutions of sulphate, nitrate, chloride, and acetate of zinc, sulphate and nitrate of copper, chloride of iron, acetate and nitrate of silver, &c.

I observed the electromotive forces of these series by Poggendorff's method of compensation, modified by Du Bois-Reymond, from a few thousandths up to one fifth of a Daniell, the latter force between very dilute and highly concentrated solutions of zinc chloride.

I give, in the following Table, the ten electromotive forces, between the combinations of two, of five solutions of sulphate of zinc, the unit being nearly 0·001 of a Daniell :—

100 parts of solution contain of $\text{ZnSO}_4 + 7\,\text{H}_2\text{O}$	15 per cent.	30 per cent.	45 per cent.	60 per cent.
1 per cent.	18	22	28	36
15 ,,	. .	5	13	21
30 ,,	. .	. .	7	17
45 ,,	. .	. .	. .	9

These numbers indicate a series of tensions; for *e. g.* the electromotive force between

15- and 30-per-cent. solution ...... is 5,
30- and 60-per-cent. solution .. 17,
15- and 60-per-cent. solution .. 21,

I then confirmed the existence of a series of tensions by connecting with each other, by four siphons, five glasses, of which the 1st, 3rd, and 5th contained solutions of equal strength (45-percent.). the 2nd contained stronger solution (60-per-cent.), the

4th weaker (15-per-cent.).   I immersed the one electrode in glass 1, the other successively in 2, 3, 4, 5.   When the second electrode dipped in 3 and 5 I obtained no current, because the concentrations of the terminal solutions were equal; but on the immersion of this electrode in glass 2, and in 4, there was always a deflection produced—in the one case by the electromotive force 9, between solutions of 45 and 60 per. cent., in the other by the force 13, in the opposite direction, between 45- and 15-per-cent. solutions.

I made the same experiments on a series of other salts, and thus determined the 15 electromotive forces between the couples formed by six solutions of cupric sulphate :—

	B	C	D	E	F
A	10	16	21	25	27
B	..	6	11	15	17
C	..	..	5	9	11
D	..	..	..	4	6
E	..	..	..	..	2

F was a solution containing, in 100 parts, 30 of crystallized salt ($CuSO_4 + 5H_2O$).   One hundred parts by volume of this solution were mixed in E with $33\frac{1}{3}$, in D with 100, in C with 300, in B with 700, in A with 2900 parts of water.

By these currents, going from the diluted to the concentrated solution, metal is dissolved in the diluted, and separated from the concentrated solution.   Only when the concentration is the same in both solutions does the current cease.

For the work accomplished by the current we should have to seek the corresponding equivalent in the work of the force of attraction between the salt and the water, which makes itself perceptible in the thermal actions which can be observed on mixing different solutions of the same salt.

Accordingly the current observed by me must be conceived as a reaction-current against the migration of the ions, as the polarization-current is one of reaction against the decomposition-current; for whenever any salt is electrolyzed, the solution becomes more concentrated at the anode, more dilute at the cathode.   My experiments show that then arises an electromotive force which acts in opposition to that of the electrolyzing battery.—*Monatsbericht der kön. preuss. Akad. d. Wissensch. z. Berlin,* Nov. 1877, pp. 674–676.

ON THE EXTRACTION OF GALLIUM.

BY MM. LECOQ DE BOISBAUDRAN AND E. JUNGFLEISCH.

The smallness of the quantity contained in the minerals in which gallium has hitherto been detected renders its preparation costly and tedious.   We proposed to ourselves to pursue a process permitting this preparation to be annexed to that of a commercial product, and thence to operate on a manufacturing-scale, on considerable masses.

The realization of this project we owe to the support of M. Léon

Thomas, who has been anxious to contribute, with generous liberality, to the success of a research of pure science. M. Thomas has kindly had treated according to our directions 4300 kilograms of Bensberg blende, that ore being the richest known.

The course adopted was as follows :—

1. The blende, pulverized, is roasted in one of the bays of a Perret oven kept at a sufficiently high temperature by the simultaneous combustion of pyrites in the other bays. The gallium remains fixed, while the greater part of the indium appears to be volatilized.

2. The product of the roasting is treated with a quantity of sulphuric acid sufficient to dissolve nearly all the zinc, leaving nevertheless in the mass enough subsulphate to cause the filtered solution to become cloudy on the addition of cold water. Thus, on the one hand, commercial sulphate of zinc is obtained, and, on the other, a residue containing gallium.

3. This residue is again taken up by excess of sulphuric acid. After reduction of the persalt of iron by metallic zinc, the filtered liquor is precipitated by carbonate of soda (fractionating and following the course of the operation with the spectroscope). The precipitates are again taken up by sulphuric acid ; then a second reduction is effected with zinc, and a fractionation with carbonate of soda.

At the Javel works all the gallium of the 4300 kilograms of blende was thus concentrated into a mass weighing (still wet) about 100 kilograms. This product was remitted to us by M. Thomas. At this point, indeed, the treatment ceased to be on the large scale, and could be pursued in a laboratory.

4. To remove the iron, which, through reoxidation, in tolerably large quantity escaped the preceding purifications, the reductions by zinc and the fractionations by carbonate of soda are repeated several times.

5. The galliferous precipitates are again taken up by sulphuric acid ; most of the excess of acid is eliminated by evaporation ; and the residue is boiled with a large quantity of water. The filter separates a deposit containing some titanic acid.

6. After purifying by sulphuretted hydrogen, to the very acid liquor, still sufficiently charged with zinc, acetate of ammonia is added, and it is again treated with hydrosulphuric gas : sulphide of zinc is precipitated, carrying with it some gallium, which is thus separated from the alumina. The additions of sulphate of zinc, acetate of ammonia, and the currents of hydrosulphuric acid are repeated as long as the sulphide gives the gallium-lines.

7. The sulphuric solution of the galliferous sulphides of zinc is carefully fractionated with carbonate of soda. Spectral examination assisting, a pretty accurate separation of the zinc is accomplished.

8. After fresh treatment with sulphuric acid in the precise proportion necessary, we separate by sulphuretted hydrogen a little cadmium, lead, indium, zinc, &c., then raise to ebullition the liquor diluted with much water. By filtering it while hot we collect a considerable amount of subsalt of gallium, which is immediately washed with boiling water ; for with cold it would redissolve in its mother-liquor.

9. The basic salt is readily attacked by potass, which leaves in the

insoluble state some iron, indium, &c. The alkaline liquor, treated with hydrosulphuric gas, then slightly acidulated with sulphuric acid, gives a precipitate consisting principally of sulphide of indium*.

10. The very slightly acid liquid being boiled with a large quantity of water, the gallium passes again into the state of a subsalt.

11. The gallium is isolated by electrolysis of the potassic solution of the subsalt. The deposition of the metal is effected advantageously only under special conditions. The intensity of the electric current, for example, should vary according to the state of the liquor; but the surface of the negative electrode must always be relatively small compared with that of the positive electrode. In one of our operations, which produced 8 grams of gallium in twenty-four hours, 40 Bunsen elements (18 centims. in height) arranged in eight parallel series, each comprising 5 elements in tension, put in action a negative electrode the double surface of which did not exceed 15 square centimetres, while the positive electrode presented an expanse of about 450 centimetres square.

The metal, when deposited cold, often forms long files of crystals resembling needles, normally fixed to the electrode by one extremity; some of them attained the length of 3 centims. Above 30° the metal trickles in drops, which collect at the foot of the electrode.

By operating in the manner above described, we collected 62 grams of crude gallium. If account be taken of the inevitable losses, and of some grams of gallium which still remain in our various products, the content of the Bensberg blende may be estimated at $\frac{1}{60000}$, or nearly 16 milligrams per kilogram. This minute proportion of material capable of extraction accounts for the operations requiring so long a time.

We purified the crude gallium by filtering it through linen of a close texture, agitating it in hot water acidified by hydrochloric acid, and repeatedly crystallizing it. From this we afterwards prepared the crystals, plates, and superfused mass of gallium which we have the honour of presenting to the Academy. The little bar was cast with metal not refined.

In an early communication we will recount various observations made in the course of our operations.—*Comptes Rendus de l'Académie des Sciences*, Feb. 18, 1878, tome lxxxvi. pp. 475–478.

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ON THE RESISTANCE OF FLUIDS.

To the Editors of the Philosophical Magazine and Journal.

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Gentlemen, Berlin, S. W. Grossbeerenstr. 24, March 3, 1878.
In your Magazine for December 1876, pages 434 and 435, Lord Rayleigh gives two formulæ (3 and 4) for the resistance of an elongated blade held vertically in a horizontal stream. These formulæ were given before Lord Rayleigh by myself, in a paper entitled " M. Thiesen, zur Theorie der Windstärke-Tafel, Sanct Petersburg, 1875," Wild's *Rep. f. Meteorologie*, J. iv. No. 9, p. 7.

<div style="text-align:center">I am, Gentlemen, yours faithfully,</div>

<div style="text-align:right">Dr. M. Thiesen.</div>

* It must be remarked that indium is but partially precipitated by potass and by sulphide of potassium.

XLVI. *On the Edge-angle and Spread of Liquids on Solid Bodies. By* G. Quincke[*].

[Plate XII.]

1. *INTRODUCTION.*—In a former communication[†] I have investigated the phenomena of capillary action at the common surface of two fluids, and have measured the capillary constant, or tension, α_{12}, of this common surface[‡] by various methods.

The fluid particles themselves must be assumed to have a ready mobility, in order that the condition of equilibrium may be rapidly attained. This is, of course, only approximately the case. The more viscous the fluids considered, and the greater the friction of the fluid particles, either of the same fluid or of the different fluids, against one another, the more slowly will the condition of equilibrium be attained. The course of the phenomena may be essentially modified from this cause.

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[*] Translated by Silvanus P. Thompson, from Poggendorff's *Annalen.*
[†] Pogg. *Ann.* cxxxix. pp. 1–89 (1870); and Phil. Mag. [IV.] vol. xli. No. 273 (April 1871).
[‡] [The subjoined extract from the memoir of 1871 referred to above explains the author's use of symbols.—TRANSL.]:—"In the following memoir I shall use the same notation as in my former communications on Capillary Phenomena (Pogg. *Ann.* 1858–69), and shall distinguish the magnitudes which relate to a point P_1 or P_2 of the free surface of the liquid 1 or 2 by means of the suffix 1 or 2, the magnitudes which relate to a point P_{12} of the common surface of two liquids 1 and 2 by means of the double suffix 1 2."

Imagine several fluids in contact with one another (for example a lenticular drop of water upon oil or mercury) and let them be gradually cooled down : the water will finally freeze. The attraction between the particles of oil or mercury and the particles of the frozen drop of water will differ only by an inconsiderable quantity from the attraction which they would have exercised upon the particles of the fluid drop of water. The common surface between the oil or mercury and the water will have similar properties whether the water be fluid or solid ; and in the common surface of oil and ice, or of mercury and ice, a surface-tension $\alpha_{1\,2}$ must exist similar to that in the common surface of oil and water or of mercury and water.

Moreover the ready mobility of the particles of oil or of mercury amongst one another, and especially their mobility with respect to the now immovable particles of water, will probably have been changed.

A similar consideration may be applied to other bodies in the liquid and solid conditions as to liquid and solid water ; and hence we arrive at the following universal proposition :—

In the common bounding surface of a fluid 2, and of a solid body 1, there exists a surface-tension $\alpha_{1\,2}$, as in the common boundary of two fluids.

This surface-tension will be the same within the fluid and within the solid body, provided only the particles are immediately on the common (geometrical) boundary of both substances. The surface-tension will be perceptible only under special circumstances in the solid body, whose particles are very difficultly movable amongst one another, but more easily in the fluid layer which bounds the surface of the solid body.

It might therefore be assumed, to return to the previously mentioned special case, that a capillary surface-tension existed not only in the capillary surface of the frozen drop of water bounded by mercury, but also in the free surface of the frozen drop bounded by air—a surface-tension which would have the same value for all points of the free surface, and which must be independent of its geometrical figure.

The fluid layer at the common bounding surface of a solid body 1 and of a fluid 2 would therefore behave as a stretched membrane having at all points a constant surface-tension $\alpha_{1\,2}$.

The action of the particles of the solid body upon a fluid particle at the point P is such as if there acted in the free surface of the solid body bounded by air, a constant surface-tension α_{1}, independent of the geometrical figure of the surface, having the same value for every fluid particle P of the intersecting line of the capillary surface.

The laws established formerly* by me, relative to the surface common to two or three fluids, may, if the foregoing considerations are just, be henceforth extended also to the case where one fluid is replaced by a solid body.

Let the three common surfaces of a solid body 1 and of two fluids 2 and 3 intersect in a curved line; then, upon a particle P in the intersecting line there act three forces lying in the plane normal to the element P of the curved line of intersection under consideration. These forces are equal to the capillary constants or surface-tensions of the three capillary surfaces, and are in equilibrium; consequently they fulfil the conditions of the equation

$$\frac{\alpha_{12}}{\sin w_3} = \frac{\alpha_{31}}{\sin w_2} = \frac{\alpha_{23}}{\sin w_1}. \quad . \quad . \quad . \quad (1)$$

In this equation w_1, w_2, w_3 represent the angles which are respectively subtended at the point P by the mutually intersecting elements of arc of the curved capillary surfaces whose directions coincide with the directions of the forces α_{12}, α_{23}, α_{31}. The symbol α_{12} represents the surface-tension or capillary constant of the surface common to the solid body 1 and the fluid 2, &c.

If a triangle be drawn (Plate XII. fig. 5) whose three sides are proportional to the capillary constants or surface-tensions of the bounding surfaces common to the solid body 1 and to the fluids 2 and 3, and which meet in a point P, then the exterior angles of this triangle give, for this point P, the edge-angles of the surfaces considered.

The triangle is possible, and has real exterior angles, only if the sum of two sides be greater than the third, or when

$$\alpha_{12} < \alpha_{31} + \alpha_{23}. \quad . \quad . \quad . \quad . \quad . \quad (2)$$

If this condition is not fulfilled, a spread of one of the fluids will take place upon the surface of the solid body.

Let us call θ_3 the acute edge-angle which the surface common to the two fluids makes with the surface common to the solid and to fluid 3; then

$$\cos \theta_3 = \frac{\alpha_{31}^2 + \alpha_{23}^2 - \alpha_{12}^2}{2\alpha_{31}\alpha_{23}}. \quad . \quad . \quad . \quad (3)$$

When the magnitudes α_{31}, α_{23}, and α_{12} are independent of the geometrical form of the surfaces, and depend only on the nature of the fluids 2 and 3 of the solid body, then *the edge-angle θ_3 is also independent of the geometrical position of the*

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* Pogg. *Ann.* cxxxix. pp. 58, 59 (1870). Phil. Mag. [IV.] vol. xli. No. 275 (June 1871), pp. 454-476.

surfaces of the solid and of the fluids, constant for all points of the intersecting line of the three bounding surfaces, and determined only by the nature of the solid and of the fluids.

Therefore the surface common to mercury and water or air, for example, makes the same edge-angle with the solid wall of a cylindrical glass tube as the surface of a drop of mercury in water or air makes with a flat glass plate on which it rests.

The second known axiom of the capillary theory concerning the constancy of the edge-angle is only a special case of that just mentioned (viz. the case when fluid 3 is air), and was first deduced, we may remark in passing, by Dr. Thomas Young[*], from considerations similar to the foregoing.

If the fluids are brought, as is repeatedly the case, into contact with solids which have a continuously curved surface without sharp corners or edges, as, for example, into a glass tube or onto a flat plate, then the surface-tensions of the surfaces common to the solid and to the fluids 2 and 3 act in opposition to each other.

Let normals be drawn to the surface of the solid 1 and to the free surface of the fluid 2, and let the acute edge-angle which is included between the normals be called θ_3; then there is equilibrium as soon as the following equation is fulfilled :—

$$\alpha_{13} = \alpha_{12} + \alpha_{23} \cos \theta_3 ; \quad \ldots \quad (4)$$

or, omitting the index 3,

$$\cos \theta = \frac{\alpha_1 - \alpha_{12}}{\alpha_2}. \quad \ldots \quad (5)$$

(see Plate XII. fig. 6).

The edge-angle θ becomes $0°$, and the fluid spreads over the wall of the tube and moistens it, as soon as

$$\alpha_1 - \alpha_{12} \gtreqqless \alpha_2 . \quad \ldots \quad (6)$$

For the case where the air is replaced by a fluid 3, the condition of spread is

$$\alpha_{13} - \alpha_{12} \gtreqqless \alpha_{23}. \quad \ldots \quad (6\,\text{A})$$

The theory developed in the preceding paragraphs will, in the sequel, be compared with experience.

2. *The edge-angle of the free surface of a fluid* must be the same for flat air-bubbles under a level plate of glass as for fluids which ascend capillary tubes of the same material.

From the whole height K, and from the vertical distance $(K - k)$ between the horizontal and vertical elements of the

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[*] Lectures on Natural Philosophy, ii. p. 658 (1807), and Young's Works, i. p. 459 *seqq.* (*Encyclop. Brit.* 1816).

meridional curve of a bubble* of a fluid 2, of specific gravity σ, under a level glass plate, are found (subject to a small correction dependent on the diameter of the bubble) the cohesion α and the edge-angle θ by the equations

$$\alpha = (K - k)^2 \frac{\sigma}{2}, \quad \cdot \quad \cdot \quad \cdot \quad \cdot \quad \cdot \quad (7)$$

$$\cos \frac{\theta}{2} = \frac{K}{K - k} \frac{1}{\sqrt{2}}. \quad \cdot \quad \cdot \quad \cdot \quad \cdot \quad (8)$$

If we call h the mean ascent, and ϑ the edge-angle of the same fluid in a glass tube of radius r, then

$$(\alpha) = \alpha \cos \vartheta = \frac{rh\sigma}{2}; \quad \cdot \quad \cdot \quad \cdot \quad \cdot \quad (9)$$

or, dividing equation (9) by equation (7),

$$\cos \vartheta = \frac{(\alpha)}{\alpha} = \frac{rh}{(K - k)^2}. \quad \cdot \quad \cdot \quad \cdot \quad (10)$$

If plates and tubes are made of the same sort of glass, the values of the edge-angles θ and ϑ found from equations (8) and (10) must be equal.

I have in a former communication † measured for equal times the heights of flat air-bubbles under a glass plate, and the elevation, in newly-drawn glass capillary tubes, for a series of simple liquids. They yield the following values of the edge-angle.

TABLE I.

Liquid.	Specific gravity. σ.	Cohesion. α.	Air-bubbles. Edge-angle. θ.	Capillary tubes. ϑ.
		mgr.	° ′	° ′
Water	1	8·253	25 32	28 48
Olive-oil.................	0·9136	3·760	21 50	29 34
Bisulphide of carbon ..	1·2687	3·274	32 16	(< 0°)
Petroleum	0·7977	3·233	36 20	37 28
Chloroform	1·4878	3·120	—	28 50
Oil of turpentine	0·8867	3·033	37 44	24 14
Alcohol	0·7906	2·599	25 12	30 35

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* Compare with M. Quincke's paper in Pogg. *Ann.* cxxxix. (1870), and Phil. Mag. [IV.] vol. xli. No. 273, p. 249 (April 1871).

† Pogg. *Ann.* cxxxix. p. 15 (1870); and Phil. Mag. [IV.] vol. xli. No. 273 (April 1871), p. 252.

Except in the case of bisulphide of carbon, where an impossible value of the edge-angle is found in the capillary tubes, the values of the edge-angle as determined by both methods agree with each other.

I obtained the same result with aqueous solutions of salts and with alcohol. For these I have combined, from innumerable experiments, the mean values of the edge-angles θ for air-bubbles and ϑ for capillary tubes in the last two columns of Table XI. of a previous communication[*]. These exhibit most discrepancy between $20°$ and $30°$.

Greater discrepancies between θ and ϑ are shown only with solutions of KCl, $MgCl_2$, $CuSO_4$, $NaNO_3$, KNO_3, and especially of carbonate of potash.

If these discrepancies might also have their origin in accidental impurities, it appeared to me nevertheless to be desirable, instead of these convenient estimations, to bring about direct measurements of the edge-angle, the more so as a much greater accuracy may be attained by the latter[†].

3. In order to measure directly the edge-angle θ, which the extreme portion of a free fluid surface makes with the level surface of a solid body, I employ the following method of reflexion:—A clean thread of glass (tubing), newly drawn out in a flame, is bent into a little siphon; the ends are cut off with a clean file, and the siphon is placed in a glass which stands upon a clean horizontal plate of plate-glass, G_1. Pass a small spirit-flame below the bend of the siphon, and the limb of the siphon sets itself exactly vertical.

If the glass be filled with a liquid, it climbs by capillary attraction up the glass thread as high as the bend; and at the opening of the vertical tube of the siphon bounded by sharp edges (see fig. 1) drops are formed with clean surfaces. The volume of these drops is almost independent of the velocity with which the drops are formed, and is equal to half the specific cohesion α_2 of the liquid in question multiplied by the periphery of the tube-wall on which the drop forms.

In most cases the drops are formed on the outer wall of the siphon-tube, so that it is possible, by the selection of glass threads of suitable internal and external diameters (usually 0.5 to 1 millim.), to get drops of suitable size to follow one another at intervals of from about 1 to 30 seconds. By shifting the

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[*] Pogg. *Ann.* clx. pp. 371–374 (1877).
[†] For mercury and transparent solid bodies the method of two reflexions formerly contrived by me is to be preferred (Pogg. *Ann.* cv. p. 40, 1858). The changes of the edge-angle observed at that time I think must chiefly be ascribed to the oil-vapours which expanded in the apparatus exhausted of air, as then used.

horizontal glass plate under the siphon-mouth the drops may be caused to fall upon different places of the plate, so that the liquid forms shallow segments of spheres with sharp circular edges.

Fig. 1 shows the contrivance as it is used. Several liquids may at the same time form drops near one another. The unused drops are caught by a square glass trough, upon the upper cut edge of which a small strip of plate-glass G_2 is laid to serve as a table. This table is set exactly horizontal with a spirit-level and small wooden wedges.

The glass plate with the drops is then placed upon a horizontal rectangular plate of plate-glass, near which is set a vertical divided circle, having a movable arm of light straw of 350 millims. length and carrying a sight of black paper. On rotating the apparatus, the sight, which has an aperture of 2 millims. diameter, describes a vertical circle, in the centre of which is the sharp edge of the drop.

A luminous flame being placed at several metres distance, two images are formed by reflexion, at the level surface of the glass and at the curved surface of the drop, due to the reflecting rays $A R_1$ and $A R_2$ (fig. 2). The first image is of natural size, the second smaller in proportion as the fluid surface is more curved. If the arm with the sight be turned forward beyond the line $A R_2$ in which the last portion of the surface of the drop reflects the light, the little image of the flame suddenly disappears; and this position is read off to the exact minute of arc, with a vernier, upon the vertical divided circle. The arm bearing the sight must be turned back through an angle 2θ in order to receive the image of the flame reflected from the level surface of the plate. The last-named position is determined once for all, and only verified as often as appears necessary; so that a single reading suffices to determine the edge-angle θ.

The exactness of this *first* method admits of being easily further increased by the employment of a telescope; yet I have found the apparatus, in the simple form described, completely adequate for my rather long-sighted eye, so long as values of θ which do not exceed $40°$ are in question.

For larger edge-angles, the method described has the disadvantage that the flame-images in the strongly-curved surface of the drops are very small and, especially in daylight, difficult to perceive.

It is therefore more convenient to measure the edge-angle by a *second* method, with a simple goniometer of the following construction.

Upon one side of a horizontal steel wire, $A A_1$ (fig. 3)

of 110 millims. length and 2 millims. diameter, is fastened with cork a mirror S (of silvered glass) of 30 millims. height and 15 millims. breadth, and upon the other side a vertical divided circle K, of 45 millims. diameter, printed on card and divided to whole degrees. An arm B C of the same steel wire serves to rotate the divided circle and mirror in a hole C D bored through a large cork which has been forced tightly onto a vertical glass rod G, of 250 millims. length and 8 millims. diameter. Two diametral arms of brass, M M$_1$, which are likewise fastened into the large cork, allow the rotation to be read off to 0°·1.

That the reflecting surface S stands parallel to the axis of rotation A A$_1$ is verified, as in the ordinary goniometer, by rotating through 180°.

The mirror S must be placed horizontal and near to a larger, straight-edged, horizontal mirror G$_2$, so that the images of a horizontal window-bar parallel to A A$_1$ reflected in each mirror may coincide. Upon the horizontal mirror G$_2$, and as near as possible to the mirror S, is laid the fixed plate with the flat drops whose edge-angle is to be determined. The eye is lowered until the reflected image of the sky-lit window has just disappeared at the curved surface of the drop; and the mirror S is turned about the axis A A$_1$ until the upper edge of the bright image of the window appears on a level with the edge of the drop. Then the mirror stands parallel to the last element of the free surface of the drop, and the rotation from the first position to the second, measured on the divided circle, gives directly the acute edge-angle θ with a precision sufficient for the purpose in question.

4. *Influence of Height of Fall and of Impurities upon the Edge-angle.*

The edge-angle θ is found to be the smaller as the height h from which the drops fall upon the plate is greater.

For water, and a glass plate carefully cleansed with alcohol, water, and a clean linen cloth, I found:—

$h=0$ millim.	20 millims.	130 millims.
$\theta=22°\ 34'$	$12°\ 44'$	$7°\ 13'$

With another plate of the same glass plate better cleansed:—

$h=0$ millim.	10 millims.	100 millims.
$\theta=12°\ 29'$	$9°\ 8'$	$5°\ 54'$

And two minutes later, repeating the experiment on another spot of the same plate:—

$\theta=16°\ 49'$	$14°$	$8°\ 41'$

When the glass was replaced by a silver plate (a mirror-glass silvered by Martin's process) the values were:—

$h=0$ millim.	50 millims.
$\theta=12°$ 49′	6° 26′

By $h=0$ must be understood a fall of the least possible height.

If the drop of water is left to evaporate, or if a portion of the water be removed by a clean thread of glass, the drop becomes thinner and the edge-angle smaller, the surface of contact with the solid remaining practically unchanged.

When with greater heights of fall the drop becomes more flattened by striking upon the flat surface, the same mass of fluid acquires a greater surface of contact with the solid. This surface of contact retains its original dimensions, and θ is found too small.

If fresh fluid is added to the drop, the surface of contact grows more slowly than the altitude of the drop, and the edge-angle acquires the same value as with minimum height of fall.

The following measurements, when it is not expressly stated otherwise, refer always to the case of minimum height of fall or of maximum edge-angle. The size, and the velocity with which the falling drops follow one another, have only a slight influence upon the edge-angle. The deviations are at least not greater than are shown by similar drops upon the most homogeneous surface possible, and seldom amount to more than 30′.

Accordingly I found when water was dropped from a wide or a narrow siphon tube upon a freshly-cleaned black glass :—

$$\theta = 6°\ 17'\ \text{or}\ 5°\ 55'.$$

After the glass had laid some time in the air:—

$$\theta = 24°\ 7'\ \text{or}\ 25°\ 15'.$$

A clean thread of glass was cut in two, and of it two siphons bent, so that the drops formed themselves on the portions that were previously united. One siphon was drawn out longer and narrower at its middle point in a clean alcohol-flame so that 10 drops of water formed on it during a minute, while upon the other 40 drops of almost the same size were formed in the same time. The edge-angle for white plate-glass then was

$$7°\ 30'\ \text{or}\ 6°\ 31',$$

according to whether the drops fell slowly or quickly.

The cleaner a surface is, other circumstances being equal, the less will the edge-angle be found.

A black glass plate cleansed with alcohol and a clean linen cloth showed for water the edge-angle

$$7°\ 34'.$$

The glass plate was rubbed with olive-oil and a clean linen cloth till all visible oil was removed; the edge-angle of the same plate for water was now

$$51° \ 55'.$$

The drop of water was then after some minutes poured off, the last traces evaporated, and after deposition of a new drop with a less surface of contact the edge-angle was

$$42° \ 10';$$

and on a repetition of the same operation,

$$31° \ 53'.$$

The glass surface behaved, therefore, like a quicksilver surface, upon which, as I have formerly shown in detail*, water has a greater or a smaller edge-angle, after it has been coated with film of foreign fluid more or less thick, provided the thickness of the film is less than $2l$, or less than double the distance at which the molecular forces of capillarity are still operative (see § 12).

In the researches here described, a portion of the film of oil with which the glass plate was coated dissolved in the water; the thickness of the film became thereby less, and a freshly deposited drop of water showed a less edge-angle.

Between the surfaces of the solid glass and the fluid mercury there obtains, however, the essential difference that the bounding surface common to water and mercury is easily displaceable, but that common to water and glass is very difficultly movable.

In the case of water and mercury, when the water is removed in portions, the normal edge-angle is forthwith restored; in the case of water and a solid, such as glass, the edge-angle becomes smaller.

In the earlier researches with surfaces of mercury†, it was possible, by applying very small quantities of oil to the free surface of the mercury or of the water, to diminish the tension of these surfaces and to give a lesser or a greater diameter to the drop of water, and a greater or a lesser value to its edge-angle.

This experiment does not succeed in the case of drops of water upon glass or any other solid body.

If the free surface of a drop of water freshly deposited upon a glass plate be touched with a glass thread moistened with oil, a portion of the oil spreads upon the free surface, but the edge-

* Pogg. *Ann.* cxxxix. pp. 66 & 72 (1870); and Phil. Mag. [IV.] vol. xli. pp. 374 & 460 (1871),

† Pogg. *Ann.* cxxxix. p. 67; Phil. Mag. [IV.] vol. xli. *loc. cit.*

angle of the water against the glass remains almost unaltered. Should an alteration take place, it is sometimes positive, sometimes negative, and seldom amounts to more than 1°. Consequently I could not detect any change of the surface of contact of glass and water.

5. Water behaves towards other solid bodies—quartz, calc-spar, mica, &c.—similarly as towards glass. Here also the edge-angle is smaller as the surface of the solid body is cleaner.

The surface of glass is never obtained clean by rubbing it with a clean linen cloth and with alcohol, nor by longer immersions in alcohol. The best course is to treat the glass with hot concentrated sulphuric acid, wash this off with distilled water, let it lie a considerable time in clean water to remove the last traces of acid, then take the plate with platinum tongs and dry it in the warm current of air above the colourless flame of a Bunsen burner.

The plates are allowed to cool upon a clean watch-glass in a large clean glass jar covered with a glass plate.

There certainly remains after this process, at the edge of the last drop that dried up, a little of the glass which had been dissolved in the water; and this coating modifies the edge-angle.

In a similar manner cut plates of quartz may be cleaned.

In the case of selenite, mica, calc-spar, and topaz, fresh surfaces of cleavage are used.

The acute edge-angle of water upon the substances mentioned is generally the greater the longer the time which has elapsed since the cleansing or the formation of the clean surface, during which the solid bodies condense upon their surface gases or vapours from the air*. Even a few seconds suffice to allow the influence, which always increases the edge-angle of the water, to become recognizable. Topaz appears to me most sensitive; less sensitive are calc-spar, glass, selenite, mica, and quartz, which last substance keeps a clean surface the longest.

Since I found it impossible to split the substance without touching it on the edge with the finger, the freshly cloven surfaces may probably in consequence have been also soiled.

I have determined the edge-angle with the cleanest possible surfaces specially for water and olive-oil, and for water with a surface rubbed with olive-oil and a clean cloth, or greasy.

The figures given are the mean of several measurements. Under " min. " are placed the smallest values which I have found in these measurements.

* Riess has formerly remarked (*Reibungselectricität*, vol. ii. p. 220) that a drop of water remains stationary upon an old surface of mica, but flows at once over a fresh one and wets it.

<div align="center">Table II.</div>

	Edge-angle.			
	Water on		Olive-oil on clean surface.	
	Clean surface.		Greasy surface.	
	° ′	° ′	° ′	° ′
		min.		
Topaz	7 37	1 58	—	14 11
Calc-spar	4 15	2 24	80	47 3
Black glass	3 2	0	18 1	—
Selenite	2 8	1 22	8 4	34 38
Mica	1 16	0	12 39	17 29
Quartz	0 55	0	7 58	10 35
Slate	0		—	24 24

A gold-leaf electroscope was immediately discharged on contact with the clean surfaces of topaz, calc-spar, glass, mica, and quartz, but not discharged, or only very slowly, on contact with the greasy surfaces of the same substances. Plates of selenite with either clean or greasy surfaces discharged the gold-leaf electroscope.

6. Metals are still more difficult to obtain with a clean surface than glass or the substances named in the preceding paraphraps.

Noble metals, as platinum and gold, in thin strips of 10 millims. breadth, are ignited in the non-luminous Bunsen's flame, and allowed to cool in a clean watch-glass between clean glass plates.

For silver I employed a film of silver deposited upon clean plate-glass by Martin's process*, which was rinsed with water as hot as possible and dried in the warm air-current over the Bunsen-flame.

Other metals were scraped with a clean knife, and the flat drops brought as quickly as possible onto the clean surfaces so prepared.

After waiting a longer or shorter time T after the preparation of the clean surface before depositing the flat drops of fluid, different values are always found for the edge-angle. In the case of water and aqueous saline solutions the difference is specially astonishing ; it is less in the case of olive-oil.

Pure alcohol and petroleum spread over a clean surface upon the whole of the metalsi nvestigated by me, and gave the edge-angle 0°.

* Pogg. *Ann.* cxxix. p. 55 (1806).

In the following Table are comprised the means of a series of measurements of water or olive-oil on the cleanest possible surfaces :—

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TABLE III.

Edge-angle for Clean Surfaces.

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Clean surface of	Water.		Olive-oil.	Water with alcohol which spreads.
	T = 2′.	T = 10′.		
Platinum	10 43	18 13	29 43	20 40
Gold	4 16	8 18	33 47	12 54
Silver	11 32	17 58	25 59	18 25
Copper	6 41	—	23 15	14 1
Lead	2 36	—	29 56	17 45
Iron	5 10	—	27 33	—
Cadmium	7 15	—	29 37	16 37
Zinc	5 52	—	33 28	13 42
Aluminium	8 11	—	23 56	15 59
Plate-glass	4 40	—	35 48	7 42

When alcohol spreads upon the clean metal surface and drives away the water already lying upon it, the water drop is driven back ; but it remains bounded by a sharp edge, and its edge-angle is increased, as a comparison of the figures of the last column with those of the second shows.

If the clean metal surface be smeared with a thin film of grease by rubbing with olive-oil and a clean cloth, the edge-angle of water or alcohol against the greasy surface is much greater than against the clean surface. Its magnitude depends on the thickness of the deposited film of oil. If the greasy surface be left some time in contact with alcohol, whereby a portion of the oil-film is removed, and the alcohol be then poured off and the remainder evaporated, water shows a smaller edge-angle on that place with the thinner oil-film.

If petroleum be smeared upon the clean metal instead of olive-oil, the water behaves towards the greasy metal surface as in the case of olive-oil. Alcohol spreads upon it and exhibits the edge-angle 0°.

Table IV.
Edge-angle on Greasy Surfaces.

Thick film upon	Thin film of petroleum. Edge-angle for water.	Thin film of olive oil. Edge-angle for		Water upon the same surface treated with alcohol.
		Alcohol.	Water.	
Platinum	66° 2′	20° 33′	44° 25′	28° 28′
Gold	38	15 6	72 10	44 52
Silver	(80)	14 5	54 48	22 10
Copper	(90)	20 12	60 54	4 55
Lead	62 30	18 14	>75	—
Iron	37 36	15 22	(85)	—
Cadmium	36 14	19 45	(80)	72
Zinc	47 42	19 15	(80)	—
Aluminium	(95)	20 4	75 55	—
Plate-glass.........	15 10	0	31 0	20 29

If once a metallic surface come into contact with oil, the adherent film of oil cannot be removed either by washing with alcohol or by immersion for a day in that fluid. The edge-angle of water against the greasy surface always remains materially greater than is yielded by the clean or fresh surfaces.

The thinner the film of oil upon the surface of the solid body, the less will be found the edge-angles of alcohol and water against the surface in question.

7. *Aqueous saline solutions* behave similarly to water in spreading upon *clean plate-glass*.

The following Table contains the means of a series of observations upon saline solutions of various degrees of concentration:—

Table V.
Edge-angle for Plate-Glass, and Aqueous Saline Solutions of various degrees of Concentration.

Substance.	Specific gravity. σ.	Amount of salt. S.	Edge-angle. θ.
Hydrochloric acid......... {	1	0	3° 9′
	1·0655	14·55	3 53
Chloride of ammonium .. {	1	0	4 15
	1·0365	13·30	9 3
	1·0737	35·18	12 42
Chloride of sodium ...... {	1	0	3 9
	1·0865	13·27	6 11
Chloride of potassium ... {	1	0	8 59
	1·0487	8·03	8 2
	1·0932	16·13	12 16

Table (*continued*).

Substance.	Specific gravity. $\sigma.$	Amount of salt. S.	Edge-angle. $\theta.$
Chloride of calcium ...... {	1	0	7 30
	1·1639	22·01	15 48
Sulphuric acid .... {	1	0	6
	1·0556	8·43	4 35
	1·2318	45·14	4 37
	1·3470	85·61	4 21
	1·5197	161·1	5 4
	1·8371	180·4	6 1
Sulphate of zinc .... {	1	0	4 25
	1·0910	9·28	9 6
	1·2187	22·59	15 7
	1·4168	45·88	20 26
Sulphate of copper ...... {	1	0	4 38
	1·0664	6·40	10 54
	1·1859	19·64	12 1
Carbonate of potassium .. {	1	0	7 30
	1·4444	72·63	14 31
Nitric acid................. {	1	0	4 38
	1·0110	2·20	5 30
	1·0915	18·08	6 59
Nitrate of potassium ... {	1	0	5 6
	1·1398	25·80	6 37
Ammonia	?	?	0° to 7° 18'
Cane-sugar .... {	1	0	5 24
	1·1170	37·67	7 8
	1·2359	102·20	9 2

I obtained similar results for other solid bodies, as platinum or gold :—

TABLE VI.

Fluid.	Specific gravity.	Edge-angle.		
		Glass.	Platinum.	Gold.
Water	1	7 30	4 53	8 11
Chloride of calcium	1·1639	15 48	15 17	7 45
Carbonate of potassium..	1·4444	14 31	8 37	7 8

According to these researches the edge-angle appears to increase a little with an augmenting concentration of the saline solution, but otherwise to differ only inconsiderably from the edge-angle of pure water.

8. Besides the direct methods described in the preceding paragraphs, I have also simultaneously determined the edge-angle indirectly against the same solid substances from the form of flat air-bubbles.

Upon the same surfaces of glass and silver, which were cleansed with alcohol, water, and a clean linen cloth, I found by *both methods* the following values of the edge-angle for *mixtures of alcohol and water* of various specific gravities:—

TABLE VII.

Alcohol of specific gravity.	With air-bubbles.		By reflexion.	
	Glass.	Silver.	Glass.	Silver.
0·9973	30° 53′	43° 31′	20° 34′	72° 14′
0·9852	26 23	62 18	15 36	62 35
0·9200	16 21	20 21	14 28	25 42

With the exception of the one determination in the case of very dilute alcohol and silver, where the fluid surface of the air-bubble was very difficultly movable, and a casual impurity may have produced a difference, the results of both methods of observation agree as far as can in general be expected in these investigations.

9. *The magnitude of the surface-tension α_{12} at the boundary of a solid and of a liquid* may be determined to within an additive constant so soon as the tension of the free surface, and the edge-angle for various liquids upon the same solid (for example glass), are known.

From equation (5), § 1, we have for the fluids 2 and 3 :—

$$\alpha_{12} = \alpha_1 - \alpha_2 \cos \theta_2, \quad . \quad . \quad . \quad . \quad . \quad (5\,\text{A})$$

$$\alpha_{13} = \alpha_1 - \alpha_3 \cos \theta_3 ; \quad . \quad . \quad . \quad . \quad . \quad (5\,\text{B})$$

or, by subtraction,

$$\alpha_{12} - \alpha_{13} = \alpha_3 \cos \theta_3 - \alpha_2 \cos \theta_2. \quad . \quad . \quad (5\,\text{C})$$

Call h the mean height of ascent in capillary tubes of diameter $2r$ for a liquid of specific gravity σ ; then, from equation (9),

$$\alpha_2 \cos \theta_2 = (\alpha) = rh\frac{\sigma}{2},$$

whence follows at once the capillary constant (α) of the free surface of the liquid concerned, as it used formerly to be calculated from the height of capillary ascent in glass tubes under the assumption that the edge-angle was zero.

Moreover the value of $\alpha_2 \cos \theta_2$ may also be calculated from observations on flat air-bubbles beneath a level plate of glass.

Comparing α_{12} for various liquids with

$$\alpha_{13} = x$$

for water as fluid 3, we obtain the following values from my earlier observations[*]:—

TABLE VIII.

Liquid.	Specific gravity. σ.	$\dot{\alpha}_2 \cos \theta_2 = \alpha_1 - \alpha_{1\,2}$.		Surface-tension against glass. $\dot{\alpha}_{1\,2} - x$.	
		Capillary tubes.	Air-bubbles.	Capillary tubes.	Air-bubbles.
		mgr.	mgr.	mgr.	mgr.
Alcohol	0·7906	2·237	2·352	4·998	5·097
Petroleum	0·7977	2·566	2·604	4·669	4·845
Chloroform	1·4878	2·733	—	4·502	—
Turpentine	0·8867	2·765	2·398	4·470	5·051
Olive-oil....................	0·9136	3·271	3·490	3·964	3·959
Bisulphide of carbon ...	1·2687	3·343	2·768	3·892	4·681
Water	1	7·235	7·419	0	0
Mercury....................	13·543	—	34·53	—	− 27·081

In this Table the liquids are arranged according to the value of their surface-tension at the boundary of glass, as follows from the observations upon capillary tubes.

With the exception of mercury, the bounding-surface of glass and alcohol exhibits the greatest, and that of glass and water the smallest surface-tension. Instead of which we may also say alcohol has the least, water the greatest adhesion to glass[†].

A similar calculation may be carried out in the case of all the aqueous saline solutions for which I have lately[‡] established the values of α. According to equation (5 c), in the case of all saline solutions for which (α) increases with augmented concentration, the surface-tension of the common bounding surface of glass and saline solution will be the less, and the adhesion of the saline solution to the glass will be the greater, as the saline solution is the more concentrated. This occurs for all the substances investigated by me, with the exception of hydrochloric acid, nitric acid, and ammonia; and it holds also for alcoholic solutions of chloride of lithium and chloride of calcium[§]. Besides, the same quantity $\alpha_{1\,2}$ may be calculated in

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[*] *Pogg. Ann.* cxxxix. p. 15 (1870); and *Phil. Mag.* [IV.] vol. xli. No. 273 (April 1871).

[†] For melted glass I have found (*Pogg. Ann.* cxxxv. p. 642, 1868) $\alpha_1 = 18·09$ mgr.; and since this value was necessarily obtained with a lowering temperature, there is nothing astonishing in the value $\alpha_1 > 34·53$, as follows from equation (5 A) and the observations on mercury. According to this equation, α_1 must always be greater than a value of $\alpha_2 \cos \theta$.

[‡] *Pogg. Ann.* clx. pp. 371–374 (1877), table xi.   [§] *Loc. cit.* p. 566.

yet another way from the observation of flat bubbles or drops of one liquid 2 in another liquid 3 or 4, beneath or upon a glass plate.

From equation (4),

$$\alpha_{12} = \alpha_{13} + \alpha_{23} \cos\theta_3, \quad \cdots \cdots \quad (5\,\mathrm{D})$$

$$\alpha_{12} = \alpha_{14} + \alpha_{24} \cos\theta_4 ;$$

or, by subtraction,

$$\alpha_{14} - \alpha_{13} = \alpha_{23} \cos\theta_3 - \alpha_{24} \cos\theta_4.$$

Let the flat glass correspond to the solid 1,
    ,,   olive-oil   ,,   ,,   liquid ... 2,
    ,,   water   ,,   ,,   ,,   ... 3
    ,,   alcohol   ,,   ,,   ,,   ... 4 ;

then according to my earlier observations[*],

$$\alpha_{23} = 2{\cdot}096 \text{ mgr.}, \qquad \alpha_{24} = 0{\cdot}226 \text{ mgr.},$$
$$\theta_3 = 17°, \qquad\qquad \theta_4 = 87° \ 48',$$
$$\alpha_{14} - \alpha_{13} = 2{\cdot}001 \text{ mgr.} \ -0{\cdot}009 \text{ mgr.};$$

or if the boundary of glass and water α_{13} is called x,

$$\alpha_{14} - x = 1{\cdot}992 \text{ mgr.}$$

According to this investigation also, the surface-tension of the level bounding surface of glass and alcohol is therefore greater than that of glass and water.

If water be taken as fluid 3, and the various liquids of observations Nos. 2, 10–14, & 16 of the former researches mentioned[†], as fluid 2, then the surface-tension of the common boundary of glass and the liquid concerned may be calculated from equation (5 D), except an additive constant α_{13}, or x.

TABLE IX.

Bounding surface of glass with	Surface-tension with glass. $\alpha_{12} - x.$
	mgr.
Bisulphide of carbon ...	4·145
Petroleum	2·815
Olive-oil.....................	2·001
Alcohol	1·992
Turpentine	0·931
Water........................	0
Mercury.....................	−38·23

The figures of the last columns in both Tables VIII. and IX.

* Pogg. *Ann.* cxxxix. p. 27 (1870) ; and Phil. Mag. [IV.] vol. xli. p. 263 (April 1871).   † Ibid.

should be identical; likewise the order of succession of the liquids.

Neither is the case; and so far the theory is not in harmony with experience.

It must, indeed, be remembered that the values of the edge-angle θ were only determined approximately with flat bubbles and drops, and can lay no claim to great accuracy—that the magnitude α_1 of the free surface of the glass may have had different values in the various researches in consequence of impurities (see § 12, hereafter)—and, finally, that merely the presence of a fluid may alter the molecular nature and therefore also the surface-tension of another, so that the density of a surface bounded by air may be quite different from that of one bounded by another fluid (compare § 11).

[To be continued.]

XLVII. *The Production of Thermoelectric Currents in Wires subjected to Mechanical Strain. By* G. W. von Tunzel-mann, *Holder of the Clothworkers' Exhibition in Chemistry and Physics at University College, London* [*].

THE following inquiry was suggested by some observations recorded in a paper of Sir William Thomson's on the Electrodynamic Qualities of Metals, in the 'Philosophical Transactions' for 1856; and the object in view was to investigate the conditions under which thermoelectric currents are produced in a circuit composed of a single metal when one portion of the metallic conductor is subjected to a strain and the junctions of the strained and unstrained portions are maintained at different temperatures.

The experiments were made upon wires of iron, steel, and copper, the copper wire employed having been obtained from Messrs. Johnson, Matthey & Co. as chemically pure.

Two tin cans were obtained open at the top, and pierced at the bottom by necks into which india-rubber corks were inserted; and through slits in these the wires were passed. The wire was fastened by a clamp in the lower can, and was grasped in the upper one by a pair of wire-drawing dogs attached to the shorter arm of a lever, to the longer arm of which was attached the weight by which the strain was produced. In the earlier experiments ordinary weights were used; but ultimately these were rejected, as it was found impossible to apply and remove them in a sufficiently gradual manner to prevent a certain amount of shock, which introduced complications.

* Communicated by the Physical Society.

In the arrangement finally adopted, there was attached to the longer arm of the lever a tin can open at the top, and having at the bottom a neck fitted with an india-rubber tube, which could be closed by merely bending it up and hitching it in a hook attached to the can for that purpose. The strain on the wire was then produced as gradually as was desired, by pouring in measured quantities of shot ; and it could be removed as gradually by letting the shot run out by the india-rubber tube.

The two cans through which the wire passed were filled with water, the water in the upper can being kept at the temperature of 100° C. by means of a gas-burner, while that in the lower can could be kept for a considerable time at a uniform temperature by allowing a current of water, of the same temperature as the place of experiment, to circulate through it.

The extremities of the experimental wire were bent round in a large curve and brought close together ; they were then tied to the extremities of two covered copper wires connected, through a four-way key, with a Thomson's galvanometer having a resistance of between one and two ohms. The junctions were then placed side by side separated by thin paper, and wrapped up in cotton-wool, as was done by Thomson in his experiments, to prevent the production of currents by the unequal heating of the two junctions.

Before being used the wires were annealed :—the iron and steel wires by being heated to redness in an iron tube, through which a current of coal-gas was passing to prevent oxidation; the copper wire by being slowly passed through a Bunsen flame, as it was found that the exposure of the copper at a red heat to the current of coal-gas produced an effect similar to that known as over-poling in the process of refining copper, the wire being rendered so brittle as to break with the least strain.

Thomson found in his experiments that when a weight was applied so as to produce a state of strain in a portion of the wire, and the two junctions of strained and unstrained portions were kept at different temperatures, in iron and steel wire a current was produced the direction of which was from the unstrained to the strained portion across the hot junction, while in copper wire the current was in the opposite direction. When the weight was removed the result was in either case a weaker current in the reverse direction.

Some experiments of the same nature have also been made by M. le Roux, and described in the *Annales de Chimie et de Physique*, 4th series, vol. x. p. 201 (1867). He obtained results of the same nature as Thomson—with the notable

difference that in iron and steel wires he got a current from strained to unstrained across the hot junction, while in copper wire the current was from unstrained to strained across the hot junction. On comparing the descriptions of the experiments, it appeared that Thomson had always made his experiments with comparatively small strains, while Le Roux had strained his wires very nearly to the breaking limit. This at once suggested a possible explanation of the apparent discrepancy between their results; and on making the experiments, it was found that as the strain was gradually increased the current was increased, as in Thomson's experiments, but only up to a certain limit. When the strain was increased beyond this limit there was a gradual decrease in the current; and if the strain was very carefully increased, the direction of the current was reversed shortly before the breaking strain was reached.

During the experiments, it was very soon observed that after a weight had been added the current did not remain constant, but gradually diminished; while at the same time there were variations of small period in the strength of the current, which were greater when the weight was added suddenly, and scarcely perceptible when it was very carefully and slowly added by pouring in shot; these variations gradually ceased when the apparatus was not disturbed. A very gentle and gradual addition of weight diminished these variations, which always died away more rapidly when there was a heavy strain on the wire. Clutching the wire in the " dogs " also set up these variations, which were allowed to subside before beginning the experiment. These results suggested that the production of the current might be due to a process of change in the molecular state of the wire; it was found, however, on examination that there was a permanent effect which could not, as far as I can see, be produced in that way, whatever may have caused the temporary effect.

Where the results obtained at different times had not to be compared, the current is generally given in terms of the deflections of the galvanometer; but where such comparison was necessary, the value of the deflections was determined at each experiment in terms of a standard current obtained by sending a current from a Daniell's cell through a definite resistance.

For the sake of brevity, U.S. will be written for " from unstrained to strained across the hot junction;" and the opposite direction of the current will be denoted by S.U. The following letters are used in the description of the experiments:—

W = tension applied to wire in pounds, = 3 times weight
     actually applied to lever;

M = number of measures of shot effective in stretching
     wire, = 3 times number actually applied to lever;

Θ = temperature of lower can;

D = mean deflection of galvanometer;

C = strength of current in terms of the standard current.

A considerable number of preliminary experiments were made to verify Thomson's results and to determine the best form of apparatus, the arrangement ultimately adopted being that already described. These experiments (which are not described here) gave a general idea of the phenomena to be looked for. The alteration of resistance from strain is not taken into consideration, as H. Tomlinson's experiments, Proc. Roy. Soc. 1876 (vol. xxv. p. 451), have shown that it is too small to have an appreciable influence upon the results.

Experiment 1.—An iron wire ·46 millim. diameter.   Θ = 16°; W = 31·5.   The result is given in the accompanying Table, the direction of the current being U.S., the first reading being taken immediately after the weight was applied. It will be observed that the current does not reach its full strength immediately upon the application of the weight, but rises rapidly to a maximum, and then gradually falls to a strength at which it remains steady.

Time (minutes).	D.
0	2·5
5	6
10	3
15	2
20	2
25	2

Experiment 2.—A similar wire. Θ = 16°. The readings were taken immediately after the application of the weight. The sign * means that there is a deflection, but too small to be measured.

W.	D.	Direction.
6	*	U.S.
9	5	U.S.
12	3·5	U.S.
15	3	U.S.
18	*	S.U.
21	{ wire broke. }	

It will be observed that the direction of the current changes just before the wire breaks.

Some steel wire was now taken ; and as it was found almost impossible by the most careful annealing to get a piece of wire arranged in the apparatus which should give no deflection before the application of the weight, the initial deflection was noted in each case.

Experiment 3.—A steel wire ·81 millim. diameter. $\Theta = 16°$. Initial deflection = 20 S.U. W = 31·5. After the application of the weight the deflection reversed its direction to U.S. The result is given in the following Table.

Time (minutes).					D
2	.	.	.	.	10
3	.	.	.	.	12
4	.	.	.	.	13
5	.	.	.	.	15
6	.	.	.	.	18
24	.	.	.	.	10
25	.	.	.	.	9
50	.	.	.	.	9
90	.	.	.	.	5
130	.	.	.	.	3

The apparatus was then left for about 90 hours with the weight attached ; Θ being still 16°, there was now a deflection of 15 S.U. The weight was then removed, and the deflection fell rapidly and changed to U.S. The subsequent deflections were as follows:—

Time from removal of strain (minutes).					D. U.S.
1	.	.	.	.	2
2	.	.	.	.	3
3	.	.	.	.	4
4	.	.	.	.	6
5	.	.	.	.	7
6	.	.	.	.	8
14	.	.	.	.	8
20	.	.	.	.	10
25	.	.	.	.	7
30	.	.	.	.	3

In both cases we observe that the current gradually rises to a maximum and then falls.

Experiment 4.—A steel wire ·98 millim. diameter. $\Theta = 15°$. Initial deflection = 10 S.U. On the addition of a weight of

42 lbs., the deflection changed rapidly to 14 U.S., and in about an hour fell to 8 U.S., and in 40 hours to 3 U.S.

Experiment 5.—A steel wire ·6 millim. diameter. $\Theta = 13°$. Initial deflection = about 1 U.S. W = 63 lbs. Direction of current after application of weight U.S. The weight was now left suspended for about 40 hours; but the apparatus received an accidental jar before the permanent deflection could be ascertained. The weight was then removed, causing a deflection of 8 S.U., gradually increasing to 11, and then decreasing much more slowly than it had increased.

Time from removal of strain (minutes).	D.
3	3·5
6	4·5
9	3·5
12	2·5
15	1·5
18	·87
36	*

The effect of rapidly putting on and taking off the weight a number of times in succession was then tried; and it was found that each time the weight was put on the deflection diminished, while each time that it was taken off the deflection was increased up to a certain limit. The immediate increase produced by taking off the weight was greater than the immediate decrease produced by putting it on ; so that on the whole there was a large increase in the current, the deflection being got up in this way to nearly 30 S.U., falling again very rapidly if the weight were left attached to the wire. Under these circumstances the deflection went down rapidly to zero, changed sign, rose to a maximum, and then again began to diminish, passed through zero in the opposite direction, and so continued to perform excursions in alternate directions, and very rapidly decreasing in extent. When the weight was permanently removed the deflection of the galvanometer died out much more slowly, and the changes of sign were only just perceptible. These phenomena confirm the conclusion to which I was led by the former experiments, that there is, besides the main effect, a transient effect produced by altering the strain on the wire ; and this transient effect appears to me to be due to the molecular state of the wire making a partial return after the first shock towards its primary condition, just as the immediate deflection of a spring suddenly stretched by a weight is greater than when it has come to rest in its position of equilibrium. The changes of sign in the current as it gradually

comes to its final state after the wire has been violently disturbed, as in the last experiment, as also the fact of there being a permanent as well as a temporary effect, seem to render this hypothesis more probable than that the current is actually produced by a change in the molecular state of the wire.

The phenomena obtained in the last experiment will be rendered more clear by a diagram.
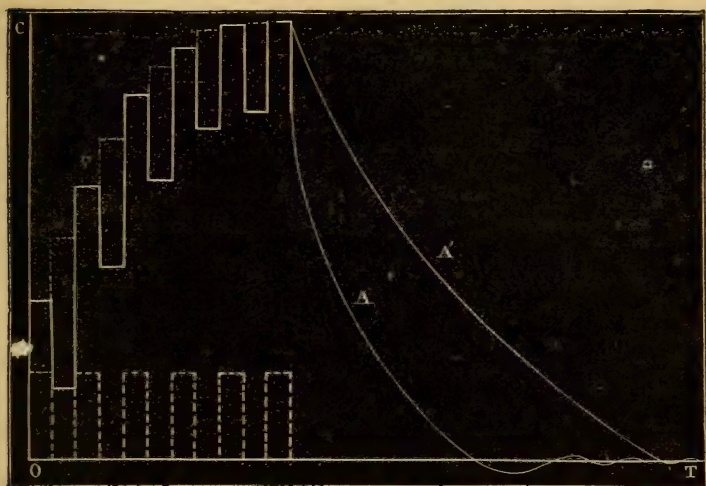
The intervals between taking off and putting on the weight were approximately equal. These are therefore represented by equal distances along the axis O T; and the strength of the current is set off along the axis O C. Starting from the point O, at the beginning of the experiment, with the weight attached to the wire as it had been left, then at any time the broken line

----- represents the permanent change in the current produced by taking off or putting on the weight ;

...... represents the temporary change ;

———— represents the resultant strength of the current, being the sum of these two components.

The curve A represents the change in the current when the weight is left permanently attached; and the curve A′ represents the change in the current when the weight is permanently removed.

At this stage in the experiments the method of measuring the current-strength in terms of a standard current was adopted.

The battery used as a standard was a " sawdust " Daniell's (Menotti's) cell; and the strength of the current was approximately that produced by 1 volt through 10,000,000 ohms, or

10^{-8} C.G.S. unit. The measurements of the Tables are given in millionths of a C.G.S. unit.

Experiment 6.—A similar steel wire. $\Theta = 16°$. Initial deflection barely perceptible S.U. Weight of 30 lbs. left on about 40 hours.

It was observed that at the time of making the experiment the weight was making small oscillations; and this appeared to be the cause of the deflections making small oscillations about a mean value. At the end of an hour and a half the oscillations of the weight and also of the deflection had ceased, the latter remaining steady at 5, indicating a current ·007 U.S. The weight was now made to perform vibrations of small amplitude, upon which the oscillations of the deflection were greatly increased both in number and amplitude, and the mean deflection was at the same time somewhat increased. If the vibration of the weight be suddenly stopped, it is some little time before a decrease is perceived in the oscillations of the deflection.

After setting the weight in gentle vibration, the effect in causing oscillations in the deflection was observable in less than a minute. If the vibrations of the weight are kept up for some time, the mean deflection is increased up to a certain limit, as before described. If the vibrations of the weight are increased in amplitude, the oscillations of the deflection become much more irregular, and the limits of variation become greater.

Experiment 7.—A similar wire. $\Theta = 12$. Initial current ·0014 U.S. A weight of 3 lbs. was now attached ; and at the end of two minutes there was a current ·0052 S.U., falling at the end of an hour and a half to ·0034 U.S. The weight was then increased by 3 lbs. at a time and the deflections taken immediately, with the results given in the accompanying Table:—

W.					C.	Direction.
6	.	.	.	.	·0038	U.S.
9	.	.	.	.	·0019	U.S.
12	.	.	.	.	·0014	U.S.
15	.	.	.	.	·0012	U.S.
18	.	.	.	.	·0012	U.S.
21	.	.	.	.	·0012	U.S.
24	.	.	.	.	·0012	U.S.
27	.	.	.	.	·0010	U.S.
30	.	.	.	.	·0002	U.S.
33	.	.	.	.	*	S.U.

In the experiments after this the weights are given in terms of measures of shot, each of which weighed about 7480 grains.

Experiment 8.—A similar wire. 33 measures left on for about 40 hours. $\Theta = 12°$. Deflections read immediately after removal of weight. Direction of current S.U. There was no initial deflection.

M.						C.
33	.	.	.	.	.	·0095
30	.	.	.	.	.	·0046
27	.	.	.	.	.	·0035
28	.	.	.	.	.	·0039
21	.	.	.	.	.	·0049
18	.	.	.	.	.	·0060
15	.	.	.	.	.	·0067
12	.	.	.	.	.	·0074
9	.	.	.	.	.	·0084
6	.	.	.	.	.	·0091
3	.	.	.	.	.	·0098
0	.	.	.	.	.	·0105

Experiment 9.—A similar wire. $\Theta = 12°$. No initial deflection. Deflections read immediately after application of weight.

M.						C.	Direction.
3	.	.	.	.	.	·0025	U.S.
6	.	.	.	.	.	·0025	U.S.
9	.	.	.	.	.	·0021	U.S.
12	.	.	.	.	.	·0014	U.S.
15	.	.	.	.	.	·0014	U.S.
18	.	.	.	.	.	·0014	U.S.
21	.	.	.	.	.	·0014	U.S.
24	.	.	.	.	.	·0014	U.S.
27	.	.	.	.	.	·0011	U.S.
30	.	.	.	.	.	·0007	U.S.
33	.	.	.	.	.	·0004	S.U.
36	.	.	.	.	.	·0014	S.U.

Experiment 10.—A steel wire ·47 millim. diameter. $\Theta = 12°$. Initial deflection 4 U.S. On attaching the empty can for containing the shot to the end of the lever the deflection increased to 20 U.S., falling to 14·5. The strain was gradually increased by pouring shot into the can until the wire broke. The deflection changed very little until the wire began to stretch, when the deflection fell very rapidly, passed through zero, and went up to about 40 S.U.

The more rapid the stretching the stronger is the current produced. When the strain was slightly lessened, so as to stop the stretching, the deflection fell very quickly to 20 S.U. On removing the strain the deflection fell rapidly, passed through

zero, and went up to 2 or 3 U.S., making irregular vibrations. The weight was replaced and additional shot poured in very slowly.   The deflection almost instantaneously changed to about 2 S.U., which increased slightly until the wire broke.

Experiment 11.—A copper wire ·24 millim. diameter. $\Theta = 15°$. Initial deflection 1 S.U.

M.						D.
0	.	.	.	.	.	1
3	.	.	.	.	.	1
6	.	.	.	.	.	3
9	.	.	.	.	.	3
12	.	.	.	.	.	3
15	.	.	.	.	.	4
18	.	.	.	.	.	4
21	.	.	.	.	.	2·5
24	.	.	.	.	.	2·5
27	.	.	.	.	.	2
30	.	.	.	.	.	1·5
33	.	.	.	.	.	1·5
36	.	.	.	.	.	1·5
39	.	.	.	.	.	1·5
42	.	.	.	.	.	1·5

The direction of the current was S.U. Several small weights were added to the can ; but the deflection remained steady at 1·5. In copper wire, no fall in the deflection was observed when the weight was left suspended for some time.

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XLVIII. *On Galvanic Currents occasioned by Differences of Concentration—Inferences from the Mechanical Theory of Heat. By* Professor Helmholtz[*].

WE will regard as the electro-chemical equivalent of an ion that amount of it which is separated at the corresponding electrode, in the unit of time, by the chosen unit of current.

The transport-number n, referred to the cation (Hittorf's $\frac{1}{n}$), gives, as with Wiedemann, that fraction of the equivalent of the cation in question which is carried by the unit of current, during the unit of time, through each cross section of the current's path in the solution, to the cathode.   On the other hand, the quantity $(1-n)$ of the anion goes in the opposite direction,

by which $(1-n)$ of the cation at the cathode becomes free—which, combined with the amount n of cation brought to this side, gives the quantity 1 set free at the cathode. In like manner the quantity n of the cation is conveyed away from the other side, by which n of the anion is set free. To this is added $(1-n)$ of the anion brought over. Now, when the cation is a metal which can deposit itself on the electrode, $(1-n)$ of the metal disappears there from the solution, and $(1-n)$ of the salt-forming acid is conveyed away; consequently from there $(1-n)$ of the salt is removed. On the other side the liberated anion combines with the metal of the electrode; and therefore 1 equivalent of new metal here enters the solution, while n of the metal is carried away and $(1-n)$ of the anion is brought over. This gives here an increase of the quantity of the salt by $(1-n)$ of the equivalent for the unit of time and unit of current. If the metal of the electrode is the same as that which is contained in the solution, the total result of the electrolysis is the same as if one equivalent of metal were carried from the anode to the cathode, and $(1-n)$ equivalent of salt in the solution from the cathode to the anode.

If, then, the salt-solution is more concentrated at the cathode than at the anode, the difference of concentration is equalized by the transfer. Therewith the liquid approaches the state of equilibrium to which the forces of attraction between the water and salt tend even in the processes of diffusion, namely the state of uniform distribution of the salt. Thus the chemical forces acting in this direction will also in turn assist the electric current acting in their direction.

That the work of the chemical forces which herewith comes in acts in this case as an electromotive force according to the same laws as other electrolytic chemical processes, can be deduced from the mechanical theory of heat.

A reversible process without changes of temperature, such as is required for the application of Carnot's law, we can institute in the following manner:—

(1) We let the quantity E of positive electricity slowly enter the anode in a constant current, and in return take away the quantity $+E$ from the cathode; or, what leads to the same result, we admit $+\frac{1}{2}E$ into the anode, and, inversely, discharge $-\frac{1}{2}E$ at the cathode. If P_k and P_a are the values of the electrostatic-potential function for the two electrodes, then is

$$E\{P_a - P_k\}$$

the work which must be done in order to bring about this through-current. If the duration of the current is equal to t, the current-intensity according to electrostatic measure is given by the equation

$$Jt = E.$$

(2). Under the influence of this through-current, in the electrolytic cell, which we suppose provided with two electrodes of the same kind of metal and filled with a solution of that metal, there is brought about a transfer of the salt in the electrolyte. The alteration hereby produced in the state of the liquid we can get rid of by evaporating, from all the layers of the liquid where the current attenuates the liquid, as much water as is conveyed thither, and, conversely, where the current produces concentration, introducing the corresponding amount of water by precipitation of vapour. If in this way the state within the liquid be maintained perfectly constant, the anion must remain wholly in its own place, because at neither end is any thing withdrawn from it, and nothing is added to it. From the cation, on the contrary, an amount perfectly equivalent to the current-intensity must pass through each cross section of the path of the current, since a full equivalent is dissolved at the anode, and precipitated at the cathode. Now, since the displacement of the anion against the water is to that of the cation against the water as $(1-n) : n$, the water must move forwards with a velocity amounting to $(1-n)$ of that of the cation. Consequently, if 1 electrolytic equivalent of the salt is combined with q parts by weight of water, and through a portion $d\omega$ of the surface the current of density i is to be led, and, expressed in equivalents, the quantity $id\omega$ of the cation, then must $q(1-n)i \cdot d\omega$ parts by weight of water pass through the same in order to keep the parts of the anode in their place.

This quantity of water, amounting to $q(1-n)i \cdot d\omega$, carries with it as dissolved constituents $(1-n)i \cdot d\omega$ equivalents of the cation as well as of the anion. Electrolysis impels through the same cross section $ni \cdot d\omega$ of the cation forwards, and $(1-n)i \cdot d\omega$ of the anion backwards; hence, on the whole, one equivalent of the cation goes forwards, and the anion remains in its place.

Therefore, if u, v, w denote the components of the electric flow parallel to x, y, z, reckoned according to the quantity of electricity which passes the unit of surface in the unit of time, the increase in the amount of water in the volume-element dx, dy, dz is, according to known hydrostatic laws, for the unit of time,

$$-\left\{\frac{\partial}{\partial x}\left[q(1-n)u\right] + \frac{\partial}{\partial y}\left[q(1-n)v\right] + \frac{\partial}{\partial z}\left[q(1-n)w\right]\right\}dx \cdot dy \cdot dz$$

$$= -\left\{u\frac{\partial}{\partial x}\left[q(1-n)\right] + v\frac{\partial}{\partial y}\left[q(1-n)\right] + w\frac{\partial}{\partial z}\left[q(1-n)\right]\right\}dx \cdot dy \cdot dz, \quad (1$$

since in the stationary current

$$\frac{\partial u}{\partial x} + \frac{\partial v}{\partial y} + \frac{\partial w}{\partial z} = 0. \quad . \quad . \quad . \quad . \quad (1\,\text{A})$$

On the contrary, at the surface of the electrodes the required inflow of water through the surface-element $d\omega$ would be

$$q(1-n)[u\cos a + v\cos b + w\cos c]d\omega, \quad . \quad . \quad (1\,\text{B})$$

if a, b, c denote the angles between the normal, directed to the liquid, of the element $d\omega$ and the positive coordinate-axes.

Integrating over the entire volume of the liquid the above expression which is multiplied by dx, dy, dz, we obtain, by known methods of partial integration, the same value that the last expression (multiplied by $d\omega$) gives when integrated over the surface.

The water, then, which collects in the whole interior, and, according to our supposition, is to be removed by evaporation, will exactly suffice, when again precipitated at the surfaces of the electrodes, to give the supply required there. Of course the collection of the water within the liquid, as well as its precipitation on the surface, may in places have also negative values.

(3) The evaporation, or, where it is negative, the precipitation of the vapour, can be managed thus:—By conveying heat to each of the volume-elements the temperature is kept constant during the evaporation. As long as water is to be extracted from a volume-element of the liquid, the vapour is left in contact with it; finally the two are separated, and the vapour, under a further supply of heat, is permitted to expand at constant temperature until it has reached a constant pressure p_1. Where the evaporation is to be negative, of course the vapour is withdrawn from the pressure p_1, and giving up heat at constant temperature is compressed at first out of, and afterwards in, contact with the liquid, until it turns to water. Since the vapour which is in contact with the more concentrated portions of the liquid has less pressure than that which is in contact with the more dilute portions, work will be gained in this evaporation when water is carried over from the more dilute to the more concentrated portions, lost when the reverse is the case.

(4) The electric current can be made to pass so slowly that the heat-development (proportional to the square of its intensity) on account of the resistance of the conductor becomes vanishingly little in comparison with those actions which we have hitherto discussed, and which are proportional to the first power of the intensity.

In like manner the diffusion which takes place between the

differently concentrated portions of the solution could be **re-
duced** to a minimum by inserting narrow connecting tubes,
without altering the electromotive force of the apparatus, which
we wish to calculate.

We can, on account of this, neglect these two irreversible
processes, and apply Carnot and Clausius's law to the reversible
ones. Since all the bodies taking part in the process are each
to have the same constant temperature, no heat can be converted
into work, nor can any work be converted by the reversible
processes into heat. The sum of the work gained and lost
must therefore, taken by itself, be equal to *nil*, as must also
the sum of the heat withdrawn and supplied. Hence result
two equations.

The one, which refers to the heat, expresses nothing but
what can be obtained without consideration of the electrolytic
process—namely, that the same amount of heat is generated
when the metal of the electrodes comes into a concentrated
salt-solution which is gradually diluted as when it enters
directly into the dilute solution.

The second equation expresses that with the above-described
reversible process the mechanical work must be equal to *nil*.
Work is expended, partly,

(1) for the collection of the electricity. If P_a and P_k are
the values of the potential-function in the anode and cathode,
and in the time t the electricity $+E$ is collected in P_k and
taken out of P_k, the work for the unit of time is, as already
remarked,

$$\frac{E}{t}(P_a - P_k) = J(P_a - P_k).$$

(2) Partly, work is performed by the expanding vapour.
This vapour is first evolved under the pressure p, which cor-
responds to the degree of saturation of the liquid with salt; it
then expands at a constant temperature up to the pressure p_1.
Naming the work for the unit of mass W, and the volume of
the unit of mass V, both referred to the given constant tem-
perature,

$$W = pV + \int_{V}^{V_1} p \, . \, dv. \quad . \quad . \quad . \quad . \quad (1\,\text{c})$$

The total quantity of this work, $\mathfrak{W}$, is found, by means of
the values shown in equations (1) and (1 B) of the current, to
be equal to

$$-\iiint dx \, . \, dy \, . \, dz \, . \, W \left\{ u \frac{\partial}{\partial x}[q(1-n)] + v\frac{\partial}{\partial y}[q(1-n)] + w \frac{\partial}{\partial z}[q(1-n)] \right.$$

$$-\int d\omega \, . \, W q(1-n)\{u \cos a + v \cos b + w \cos c\} = \mathfrak{W}. \quad (2)$$

By partial integration of the triple integral, and taking into account equation (1 A), we find

$$\mathfrak{W} = \iiint dx \cdot dy \cdot dz \cdot q \cdot (1-n) \left\{ u\frac{\partial W}{\partial x} + v\frac{\partial W}{\partial y} + w\frac{\partial W}{\partial z} \right\}. (2 \text{A})$$

Here n and W are functions of q. Consequently, if we put

$$q(1-n)dW = d\Phi, \quad \ldots \quad \ldots \quad (2 \text{B})$$

where Φ denotes a function of q, or

$$\Phi = \int_{p_0}^{p} q(1-n)\frac{dW}{dp}dp, \quad \ldots \quad \ldots \quad (2 \text{C})$$

in which p, the pressure of the vapour above the salt-solution, is likewise a function of q, we get

$$\mathfrak{W} = -\int d\omega \cdot \Phi\{u \cos a + v \cos b + w \cos c\}. \quad (2 \text{D})$$

The parenthesis in this expression denotes the component of the current perpendicular to the limiting surface of the electrolyte. This differs from *nil* only at the parts of the limiting surface turned to the electrodes. If the concentration of the liquid, and therefore q, u, p, Φ, along each single electrode is constant, then becomes

$$\mathfrak{W} = J(\Phi_k - \Phi_a), \quad \ldots \quad \ldots \quad \ldots \quad (3)$$

and the equation of the work becomes

$$P_k - P_a = \Phi_a - \Phi_k = \int_k^a q(1-n)\frac{dW}{dp}. \quad \ldots \quad (3 \text{A})$$

But $P_k - P_a$ is the value of the electromotive force produced by the electrolytic cell in the direction from the anode to the cathode, consequently in the direction of our assumed current.

This equation therefore indicates the existence of an electromotive force, the amount of which depends only on the concentration of the liquid at the two electrodes, not upon the distribution of more concentrated and more diluted layers in the interior of the liquid—a conclusion which is confirmed by the experiments of Dr. J. Moser, recently communicated to the Academy.

At the temperature of the apartment, the diminution of pressure shown by the vapour over the solutions of most of the metallic salts is very inconsiderable; and on this account the quantity $\frac{\partial W}{\partial p}$ may be supposed approximately constant within these narrow limits of the pressure. It can therefore be placed before the symbol of integration. On the other hand,

according to Wüllner's experiments the diminution of the vapour-pressure is to the amount of salt dissolved in the constant quantity of water directly, and therefore to our q inversely proportional. If we use p_0, hitherto left undefined, to denote the vapour-pressure of pure water at the temperature of the experiment, we may put

$$p_0 - p = \frac{b}{q}, \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (4)$$

where b denotes a constant depending on the kind of the salt. Consequently

$$P_k - P_a = b \frac{\partial W}{\partial p} \int_{p_k}^{p_a} (1-n) \frac{dp}{p_0 - p}. \quad . \quad . \quad (4\,\text{A})$$

In intervals in which $(1-n)$ has a constant value this would become

$$P_k - P_a = b(1-n) \frac{\partial W}{\partial p} \log \frac{p_0 - p_k}{p_0 - p_a} \quad . \quad . \quad (4\,\text{B})$$

$$= b(1-n) \frac{dW}{dp} \log \left(\frac{q_a}{q_k} \right). \quad . \quad . \quad . \quad . \quad (4\,\text{C})$$

The quantity $\dfrac{\partial W}{\partial p}$, here occurring, has at all events a positive value. If we suppose Mariotte's law valid for the inconsiderable densities possessed by the aqueous vapour at the temperature of the apartment, and if V denotes the volume of the mass-unit of the vapour under the pressure p, then (as noted above in equation 1 c) is

$$W = p V + \int_V^{V_1} p \,.\, dv.$$

According to Mariotte's law,

$$V = \frac{V_1 p_1}{p},$$

$$dV = - V_1 p_1 \frac{dp}{p_2},$$

$$\int_V^{V_1} p \, dv = V_1 p_1 \log \left(\frac{p}{p_1} \right),$$

$$W = p_1 V_1 \left\{ 1 + \log \frac{p}{p_1} \right\},$$

$$\frac{\partial W}{\partial p} = \frac{p_1 V_1}{p} = V \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (4\,\text{D})$$

as an approximately correct value.

Hence it follows that the electromotive force of the cell is positive if the liquid is more concentrated at the cathode and consequently $q_k < q_a$ and $p_k < p_a$, which is likewise confirmed by a great number of observations by Dr. J. Moser.

For inconsiderable concentrations, and correspondingly slight diminutions of the pressure of vapour above the solution, formulæ (4 c) and (4 d) give also *the law of the increase of electromotive force with rising concentration* of the solution, since the value of $(1-n)$ is, according to Hittorf's investigations, nearly constant for slight concentrations, but rises for greater ones.

The S of the following Tables is the quantity of water (proportional to q) which is combined with the anhydrous salt in the solution; A is the electromotive force, according to the observations of J. Moser, stated in thousandth parts of a Daniell element (Cu, $CuSO_4$, $ZnSO_4$, Zn). The quantity

$$\eta = \frac{1}{A} \log \frac{S_k}{S_a}$$

should, according to equation (4 c), be constant.

For a cell with sulphate-of-copper solution and copper electrodes the following values are found.

Sulphate of Copper.

S_k.	S_a.	A observed.	A calculated.	η.	Value of $1-n$ according to Hittorf.
128·5	4·208	27	27	0·0550	0·724 for S = 6·35
—	6·352	25	23·75	0·0552	
—	8·496	21	21·45	0·0562	
—	17·07	16	15·94	0·0548	
—	34·22	10	10·45	0·0575	0·644 for S > 39·67

As calculated values of A those are given which are obtained when the value of q from the first observation is retained for the others also. With sulphate and chloride of zinc, which can be employed in more concentrated solutions, greater deviations in these values occur[*] simultaneously with great increase in the values of $(1-n)$.

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[*] *Note added Jan.* 1878.—More recent experiments by Dr. Moser show, indeed, that with zinc chloride the quantity $\dfrac{\partial W}{\partial p} = V$ increases to half as much again with greater concentrations, and can no longer be regarded as approximately constant.

Sulphate of Zinc.

S_k.	S_a.	A observed.	A calculated.	η.	Value of $1-n$ according to Hittorf.
163	1·972	36	29	0·0543	
—	2·963	28	26·4	0·0635	0·778 for S = 2·524
—	4·944	22	23·1	0·0707	0·760 for S = 4·052
—	10·889	18	18·0	0·0673	0·636 for S = 267·16

Chloride of Zinc.

99	19	21·5	24·7	0·0333	0·70 for S = 332·87
—	9	40·4	36·0	0·0258	
—	5·66	42·9	42·9	0·0290	
—	2·33	67·1	56·2	0·0243	1·08 for S = 2·774
—	1·22	120·9	65·9	0·0158	
—	0·67	200·0	......	0·0108	

The great deviations which occur, especially with the higher concentrations, may probably be accounted for partly by the rise in the value of $(1-n)$ for the denser solutions, and partly by the more considerable diminution of the vapour-pressure. As the laws of both alterations for these salts have not yet been investigated, I could not institute a more detailed calculation.

Respecting the *calculation of the absolute value of the electromotive force* we have further to remark as follows :—The current-intensity J hitherto used is measured electrostatically ; likewise the electromotive force $P_k - P_a$ is determined in electrostatic units. Measured in electromagnetic measure the current intensity J will become

$$\mathfrak{J} = \frac{1}{\mathfrak{C}}\, J,$$

and the electromotive force

$$\mathfrak{A} = \mathfrak{C}(P_k - P_a),$$

where $\mathfrak{C}$ is the velocity determined by W. Weber. According to the determinations of Friedrich Weber the electromotive force of a Daniell's element (Cu, $CuSO_4$, $ZnSO_4$, Zn) is, in electromagnetic measure,

$$\mathfrak{A}_D = 109540000 \frac{\text{centim.}^{\frac{3}{2}}\ \text{gram.}^{\frac{1}{2}}}{\text{second}^2}.$$

Now W. Weber's electromagnetic current-unit, the unit of which is

$$\frac{\sqrt{\text{milligr. millim.}}}{\text{second}} = 0\cdot01\frac{\sqrt{\text{gram centim.}}}{\text{second}},$$

decomposes, according to R. Bunsen, 0·0092705 mgr. of water and $\frac{159·5}{18}$ times as much sulphate of copper—that is, 0·082147 mgr.

If, then, as in the numerical Tables, we denote by S the amount of water contained with one part by weight of anhydrous salt in the solution, for the experiments with sulphate of copper

$$\mathfrak{E}q : \mathrm{S} = 0·0082147 \text{ sec.} \sqrt{\frac{\text{gram}}{\text{centim.}}} : 1.$$

Now, if the diminution of the vapour-pressure by the salt-solution employed is known, we obtain the constant b from the equation

$$p_0 - p = \frac{\mathfrak{E}b}{\mathfrak{E}q},$$

in which the pressure p is also to be reckoned in absolute force-measure as $\dfrac{\text{gram}}{\text{centim. sec.}^2}$.

Our equation (4 c) becomes

$$\mathfrak{A} = \mathfrak{E}(\mathrm{P}_k - \mathrm{P}_a) = (\mathfrak{E}b) . \mathrm{V}(1-n) \log\left(\frac{\mathrm{S}_a}{\mathrm{S}}\right).$$

Consequently the value of the constant $\mathfrak{E}$ need not be known for the calculation of the $\mathfrak{A}$'s in electromagnetic measure.

Since we have assumed for the vapour the validity of Mariotte's law, the product

$$\mathfrak{E} . b . \mathrm{V} = \mathfrak{E}q . p_0 . \mathrm{V}_0 . \frac{p_0 - p}{p}.$$

The ratio $\dfrac{p_0 - p}{p}$ is, from Wüllner's experiments, in many salts nearly constant with changed temperature; while the product $p_0 . \mathrm{V}_0$ increases approximately in proportion to the absolute temperature, which, within the limits of the temperature of the apartment, is not of much consequence. In fact the experiments do not show any considerable influence of the temperature upon the electromotive force of the cells; at least it by no means varies in so great a proportion as the pressure of the saturated vapour.

For testing the accordance of the absolute value of the electromotive force of our series with that given by the formula, sufficient data on the vapour-tension of the salt-solutions used are still wanting. If we employ equation (4 c) in order to calculate, from the electromotive force found by J. Moser for

cells with sulphate-of-copper solutions, the quantity $\dfrac{p_0 - p}{p_0}$ for the one-per-cent. solution at 20° C., we find this quantity equal to 0·00082, while M. Wüllner[*] has found the same quantity,

For cane-sugar .... = 0·00070,
For nitrate of potass... = 0·00229,
For sulphate of soda... = 0·00236.

From the chemical properties of sulphate of copper it is probable that, in this respect, it takes its place between cane-sugar and the alkali-salts[†]. Experiments are in preparation in the laboratory here for the purpose of obtaining more accurate determinations. Meanwhile this calculation shows already at least so much, that the consideration instituted gives a theoretic value of the electromotive force which is of the same order of magnitude as the observed.

Since, moreover, factors obtained from the most various kinds of physical investigations, and one of which amounts to above a hundred millions, must be eliminated from both sides of the equation, this preliminary result is still of some importance.

XLIX. *Rate of Earthquake-wave Transit.* *By* R. Mallet, *F.R.S.*[‡]

I PRESUME that I have been indebted to the politeness of General Abbot, U.S. Engineers, for a copy of a paper by that officer, published in the 'American Journal of Science' for March 1878. In this paper the writer recurs to his account of the experiments made at Hallet's Point on the occasion of the great explosion there, on the rate of seismic-wave transmission as described in General Abbot's paper read before the American National Academy of Sciences, October 18, 1876, and also published as one of the papers of the Essayons Club of the Corps of U.S. Engineers. Upon the results there recorded I deemed it necessary to publish some remarks in the Philosophical Magazine for October 1877, in which I pointed out their anomalous character and their entire discordance with each other. If I rightly gather General Abbot's meaning from his last paper above alluded to, he considers that the

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[*] Poggendorff's *Annalen*, vol. ciii. p. 556.

[†] *Supplementary Note* (Jan. 1878).—Dr. J. Moser has since effected determinations of the quantity in question, employing water and dilute solutions instead of mercury. He obtained 0·00086 as the mean value from three experiments.

[‡] Communicated by the Author.

enormous discrepancies between the results of observations made at different points along the range from Hallet's Point are reconcilable by taking into account the difference in magnifying-power of the different seismoscopes there employed. It seems to me, however, that this proposed explanation, if critically examined, would be found wholly insufficient to account for the enormous discrepancies between the observations made at the several stations, still less to reconcile the transit-velocities recorded with the well-established and interdependent physical conditions of the transit of sound, or analogous elastic waves, in liquids and solids.

Few physical data have been better established experimentally than the rate of transit of sound in water (approximately about 4700 feet per second), as determined by Colladon and Sturm in the Lake of Geneva, and confirmed by Wertheim by a different method. Yet the transit-velocity in discontinuous water-logged shingle is given by observation No. 5 at 5309 feet per second; and it is suggested that the rate is increased by the presence of the water. How these conditions are reconcilable with each other, or with the well-known physical conditions by which the circumstances of transmission of sound are interdependent and linked together, I am unable to imagine. The proposed smoothing-down of the discrepancies by referring them to the differences in magnifying-power of the seismoscopes employed is insufficient to account for discrepancies so enormous ; and the proposed explanation seems to me only to amount to this—that if different observers note the instant of arrival as indicated by different parts of the same seismoscopic wave, they will necessarily obtain discordant results, and such as, in my judgment, no ingenuity of discussion of the observations recorded can reduce to the position of reliable scientific data.

It is with surprise and disappointment that I find General Abbot has not acquainted himself with the magnifying-power of the seismoscope constructed by me and employed in all my experiments; and so little does he seem to have acquainted himself with the scientific literature of the subject before he himself commenced to work upon it at Hallet's Point, that I am compelled to suppose my published description of that instrument, and all my earlier researches, which were not communicated to the Royal Society, but were published by the British Association for the Advancement of Science, remain even now unknown to him. My seismoscope, which is that which I have employed in all my subsequent researches, General Abbot will find described by referring to my " Second Report on the Facts of Earthquake Phenomena,"

printed in the British-Association Reports for 1851, and accompanied by an engraving (plate 13) of the instrument. The circumstances are the more extraordinary, because my seismoscope and its magnifying-power are also actually described in the account of my experiments made in the rocks of Holyhead, printed in the 'Philosophical Transactions' for 1861, with which General Abbot seems to have been acquainted. He will there find also, at page 279, that the magnifying-power of my instrument is 22·78, or nearly 23 times; and this degree of magnifying-power I have found sufficient under all circumstances. I may notice, however, that in some experiments made a few years since by me, at the desire of Sir G. B. Airy, Astronomer Royal, of Greenwich, in concert with the then chief assistant of that observatory, Mr. E. J. Stone, now Astronomer at the Cape of Good Hope, and with Mr. Carpenter, one of the Greenwich computers, a seismoscope identical in principle with my own, but of much greater magnifying-power, was employed, the power being capable of modification by changing eyepieces. Any determination of wave-transit velocities was beside our object; but this was remarked distinctly, that changing the magnifying-power of the instrument produced no noticeable change in transit-velocity as indicated by it; nor was any such, I believe, noticed by the late Sir James South in his experiments, made many years ago, to determine the extreme radius of the area caused to vibrate by railway trains passing through Kilsby tunnel. In all these instances, however (except possibly that of Sir James South, as to which I possess no details), the seismoscope was used in the only way by which it can afford trustworthy and comparable results as to the instant of transit of the seismic wave as seen in the instrument, namely by bringing the horizontal wires of the illuminating achromatic object-glass parallel and near to the horizontal wire of the observing-telescope, and always noting and adopting as the instant of wave-transit the instant at which the image of both these wires became rapidly blurred or confused and suddenly invisible. This method, which does not seem to have been adopted in any of the Hallet's-Point observations, is greatly to be preferred over any supposed observation of the earliest access to view of the front slope of the advancing wave, which, in reference to time, must always be a matter of great uncertainty. As to the duration of the vibratory disturbance in the field of view of the instrument, to which importance seems to have been attached in the Hallet's-Point experiments, it is quite delusive as affording any precise or useful information as to the dimensions or time occupied in the transit of the earth-wave or wave of shock

itself—the duration of sensible disturbance as seen in the instrument being much dependent upon the dimensions, form, material, and other details of construction of the mercury-trough and other parts of the instrument. All the observations of velocity of wave-transit referred to by General Abbot were made in completely discontinuous material, or in rock more or less water-logged, and with the directions of stratification, lamination, and fissuring imperfectly known and not recorded.

If General Abbot will refer to my experiments at Holyhead (Phil. Trans. 1861 & 1862), he will find the enormous retardation of transit-velocity produced by fissuring and discontinuity, amounting in some instances to an extinction of $\frac{7}{8}$ of the velocity in the material of the rock if perfectly unfissured. The velocity of wave-transit, therefore, in absolutely discontinuous shingle can be but a small fraction of that which the material of the shingle itself could transmit. General Abbot may satisfy himself of this by experiment upon this shingle by methods altogether independent of the use of the seismoscope. The velocity recorded in observation No. 5— 5309 feet per second (Amer. Journ. p. 179), cannot, as it seems to me, have been derived from the discontinuous shingle, and seems more likely to be an over-rated velocity of the wave derived from the water itself. Yet the velocities supposed to be obtained approach, in most instances, those given as the results recorded by the greatest experimental physicists for the velocities of sound in media as uniform, dense, and elastic as are many metals. If General Abbot will consult the works of Wertheim, of Biot, not to name other renowned physicists, and will compare the sound-velocities in solids as given by them with those recorded in relation to the Hallet's-Point experiments, I think he will see grave reason for doubt, at least, as to the validity of the latter. To me, indeed, it seems that, if they are to be accepted without further and radical modification or explanation, we must cast aside nearly all that has been accepted and is still held true as to the doctrine of sound by all men of science since the time of Newton; we must also cast aside the deductions, as to the rate of transit of earthquake-shock, derived from observation by Professors Schmidt and Nöggerath in earthquakes extending over large areas in Hungary and in Rhenish Germany, as well as those by myself of the great Neapolitan shock of 1857. Omitting the last, as the time-measures were not free from doubt in some instances, those of Schmidt and Nöggerath may be relied upon as made with much care and exactitude. All these results square as nearly as was to be expected with those of

my own experimentally obtained velocities in various rocks long previously obtained, but appear to me wholly irreconcilable with those assumed as resulting from the Hallet's-Point observations. Is this probable? or is it not much more likely that some grave and still undiscovered sources of fallacy and error exist in these experiments, on which I have felt it incumbent on me thus to animadvert?

Physical infirmity has prevented my examining the subject with that fulness I could have desired; loss of sight has compelled me to confine myself to placing before General Abbot, and scientific men in general, some of the difficulties which his Hallet's-Point experiments present to me and, I must suppose, to all competent physicists.

L. *On the Nebular Hypothesis.*—VIII. *Criteria.* By PLINY EARLE CHASE, *LL.D., S.P.A.S., Professor of Philosophy in Haverford College.*

THE views of astronomers, respecting the mode of action in world-building, have been various and vague. No one appears to have put upon record any numerical calculations, undertaken with a view crucially to test the nebular hypothesis, or any suggestions as to the proper way to make such calculations.

Statements have been made, at different times, by investigators who thought that observed velocities might be explained by the results of nebular condensation; but no one, except Ennis*, has given us any means of judging on what grounds the belief rested. It seems likely that they all looked upon the formation of planetary rings as a merely superficial phenomenon, that their studies were limited to the direct action of living forces, that they used no adequate criteria for distinguishing between nebular and meteoric influences, and that their methods often, if not always, virtually assumed the very principles which they sought to prove.

Herschel†, somewhat obscurely, intimated the possibility that nuclei might be simultaneously formed at different depths within the body of the nebula, by the action of particles of different densities; Peirce, Alexander, Hill, Wright, Kirkwood, and myself discovered various planetary harmonies which point unmistakably to such synchronous internal and external activities; yet no one seems to have thought of the likelihood that interior portions would acquire a greater

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* 'Origin of the Stars'; and Phil. Mag. April 1877, pp. 262–271.
† Outlines of Astronomy, §§ 871–2.

angular velocity than the nebular nucleus, so that a planet might revolve in less time than its sun rotated, or a satellite in less time than its primary, until I called attention to the fact that the time of nucleal rotation must vary as the $\frac{4}{5}$ power of the time of superficial nebular revolution.

The significance of this relation does not seem, even now, to be generally understood; for when Professor Hall found the unprecedented rapidity with which the inner satellite of Mars actually revolves, some thought that he must have made a mistake in his calculations, and others assumed that the discovery was fatal to the nebular hypothesis. It may therefore be a fitting time for an explicit statement of some obvious evidences of present nebular activity, such as are shown in the following comparative synopsis:—

$M \div$	n	n^2	$n^3 = \delta_2 = 2\,♀_2$
π	$\pi n = 2\ \ ☿_3$	$\pi n^2 = ♃_3 = 2\left(1^{01}\right)$	
π^2	$\pi^2 n = ♁_3 = 2\,♄_3$	$\pi^2 n^2 = \delta_4$	
π^4	$\pi^4 n = 2 \oplus_2$	$\pi^4 n^2 = \odot \rho_0$	
$\pi^7 = ♀_3 = 2\,☿_3$			

M = present modulus of light at Sun's surface = 2204·95 × Earth's semiaxis major. I have already shown the importance of this quantity, (1) by identifying the velocity of light with the limiting velocity toward which the mean solar centrifugal and centripetal forces both tend, (2) by showing that the same harmonic progression is manifested in the Fraunhofer lines and in planetary distances, (3) by tracing numerous harmonic arrangements among spectral lines of chemical elements. M is the common dividend for all the planetary positions; the combinations of various powers of π and n are divisors.

π = ratio of circumference to diameter, and also, as I have shown, ratio of incipient to complete dissociative force.

$n = \dfrac{2}{3 - 2\sqrt{2}} = 11 \cdot 65684^*$. I propose to call this quantity "Gummere's criterion," because I obtained it by a calculation which was first suggested by a criticism of Samuel J. Gummere, late President of Haverford College. The criticism, together with Ennis's rejoinder, may be found in Appendix II. to 'Origin of the Stars.'

* Phil. Mag. October 1877.

$\rho_0 =$ Sun's present nebular radius, or the distance at which planetary revolution and solar rotation would be synchronous.

The subscript figures denote apsidal positions: $_1$, secular perihelion; $_2$, mean perihelion; $_3$, mean; $_4$, mean aphelion; $_5$, secular aphelion.

The multiple 2 denotes the primitive nebular radius which would give the *vis viva* of circular-orbital revolution, by condensation to the present planetary radius vector.

It should be noted:—that critical positions of all the planets, together with some asteroidal positions, are represented in the Table; that all the symmetrical combinations of π and n, which are embraced in the Table, have planetary representations; that both of these rupturing-factors seem to have been simultaneously operative; that, after the first conversion of linear into circular motion, the exponential increments of π form a figurate series; and that the relations have all been found, not by happy guessing, but by following indications which are mathematically deducible from the necessary action of central forces.

The character of the accordances is shown in the following Table:—

Divisor of M.	Quotient.	Fact.		Minimum error.	Maximum error.
πn	60·210	$2 \Psi_3$	60·068	$+ \cdot 142$	$+ \cdot 142$
$\pi^2 n$	19·165	$\begin{cases} \text{☄}_3 \\ 2 \text{♄}_3 \end{cases}$	19·184 19·078	$- \cdot 019$ $+ \cdot 087$	$- \cdot 019$ $+ \cdot 087$
πn^2	5·165	$\begin{cases} \text{♃}_3 \\ 2(101) \end{cases}$	5·203 5·168	$- \cdot 038$ $- \cdot 003$	$- \cdot 038$ $- \cdot 003$
$\pi^2 n^2$	1·644	♂_4	1·644	$\cdot 000$	$+ \cdot 120$
$\pi^4 n$	1·942	$2 \oplus_2$	1·932	$+ \cdot 010$	$- \cdot 058$
n^3	1·392	$\begin{cases} \text{♂}_2 \\ 2 \text{♀}_2 \end{cases}$	1·403 1·396	$- \cdot 011$ $- \cdot 004$	$- \cdot 132$ $- \cdot 054$
π^7	·730	$\begin{cases} \text{♀}_3 \\ 2 \text{☿}_3 \end{cases}$	·723 ·774	$+ \cdot 007$ $- \cdot 044$	$+ \cdot 007$ $- \cdot 044$
$\pi^4 n^2$	·167	$\odot \rho_0$	·167	$\cdot 000$	$\cdot 000$

The importance of my introduction of various apsides into the study of planetary harmonies has been fully recognized by Alexander, the Nestor of harmonic astronomy; but in order to avoid all possible cavil I give the maximum errors, or deviations from the semiaxis major, as well as the minimum errors, or deviations from the nearest apsis.

The next Table gives the results of internal rupture which are indicated by Gummere's criterion, starting from the theoretical origin of Neptune's present orbital *vis viva*. In each instance the theoretical angular velocity of revolution, for the

dense inner planet, must have been $(11 \cdot 65684)^{\frac{3}{2}}$ times as great as the angular velocity of the undisturbed portions of the gasiform rotating nebula. The common divisor for the quantities in the dividend column is n.

Dividend.	Quotient.	Fact.
2♆$_4$	5·204	5·203 = ♃$_3$
♆$_3$	2·576	2·577 ⑨③
♄$_4$	1·760	1·736 ♂$_5$
♄$_3$ 2♄$_3$	1·646 } 1·637 }	1·644 ♂$_4$
2♃$_4$	·931	·932 ⊕$_1$
♄$_2$	·779	·774 ♀$_5$
♄$_1$	·749	·749 ♀$_4$
♃$_5$	·473	·477 ☿$_5$
♃$_3$	·446	·455 ☿$_4$

The great density of Jupiter as compared with Neptune, the great density of the intra-asteroidal as compared with the extra-asteroidal planets, the position of Earth in the centre of the belt of greatest planetary condensation, the rupturing-relation (n) between the positions of Jupiter's incipient and Earth's complete condensation, the fact that Jupiter is the largest extra-asteroidal, while Earth is the largest intra-asteroidal planet, the further evidence of a primeval intimate connexion between Jupiter and Earth which is furnished by the equivalence of their dissociation velocities, the probability, so far as we can judge from Sun's present nebular radius (ρ_0), that all the planets were formed when their orbital revolution was accomplished in less time than the rotation of the solar nucleus—all point to increments of wave-velocity and of centripetal velocity as sources of interior nebular rupture, giving a new meaning to Herschel's doctrine of "subsidence," and making the inner moon of Mars a confirmation, rather than a formidable objection, to the nebular hypothesis.

Adams and Leverrier found, as the result of their calculations, that the disturbances of Uranus might be explained by the action of a planet, of a given size, in a certain position. The planet was found nearly in the direction indicated, but at three fourths of the anticipated distance, and having about three fifths of the anticipated magnitude. The laws of gravitation do not determine the reciprocal perturbations of cosmical orbs by a more inexorable mathematical necessity than that which connects the activities of M, π, and n in an elastic medium like the hypothetical luminiferous æther.

The tendency to synchronous oscillations under the action of central forces, which Laplace, Peirce, and Kirkwood have

happily adduced in explanation of some of their planetary harmonies is shown (1) in the synchronism of planetary revolution at Sun with the passage of a light-wave through the major axis of the Uranus-Earth ellipse, (2) in the synchronism of solar rotation with the passage of a light-wave through the major axis of the modulus-atmosphere.

The following Table represents theoretical stages of nebular condensation, based upon forces which are now operating within the solar system. It shows some new and interesting relations between three cardinal planetary centres, viz. the centre of greatest annular condensation ($\oplus$), the centre of planetary inertia (h), and the centre of incipient systemic specialization (Ψ).

$r \div r_0$.	$\rho \div \rho_0$.	ρ.	Fact.	E.
2^3	2^4	$2.667\,r_1$	$2.667 = \frac{8}{3}\oplus_3$	.000
			$2.637 = \sqrt{3}\,\oplus_1$	.011
			$2.614 = \Psi_5 \div n$	.020
			$2.780 = h_1 \div \pi$	.043
3^3	3^4	$13.500\,r_1$	$13.490 = \sqrt{2}\,h_3$	.001
4^3	4^4	$42.667\,r_1$	$42.474 = \sqrt{2}\,\Psi_3$	.005
$214.86 = r_1$	$214.86^{\frac{4}{3}}$	$46083.4\quad r_0$	$46164.7 = 214.86$	.002
$2049.51 = h_3$	$2049.51^{\frac{4}{3}}$	$932262\quad r_0$	$947511 = 2M$	.016
$6453.06 = \Psi_3$	$6453.06^{\frac{4}{3}}$	$4302218\quad r_0$	$4263801 = 9M = [*] \div n$	.009

In this Table r_0 = present solar nucleal radius; r = past nucleal radius; r_1 = Earth's semiaxis major; ρ_0 = present nebular radius; ρ = past nebular radius; E = ratio of error, found by dividing the difference between ρ and fact by ρ; [*] = stellar distance with parallax $0''\cdot 89$, which is of the same order of magnitude as the distance of α Centauri. It is further worthy of note, that Earth's position is a mean proportional between the nebular radius, when Sun's nucleus reached the Earth, and Sun's present surface; that the nebular radius of the Jupiter-nucleal Sun was $\frac{1}{2}$ of $\frac{2}{3}$ of $\frac{2}{3}$ of M; that the nebular radius of the Uranus-nucleal Sun was nearly $5\,M$ ($4\cdot 996\,M$); that the three outer nebular radii were thus in figurate progression $(2, 5, 9)$; and that M, when Sun was expanded to the outer portions of the asteroidal belt, was coincident with [*], the origin of the incipient wave-condensation of the nebular radius of the Neptune-nucleal Sun.

The ratio $(28 : 1)$ which I pointed out at the close of my paper on momentum and *vis viva*[*], and the important part which water plays in the armoury of our globe, suggest a comparison between the liquid and the vaporous states. The

* Phil. Mag. October 1877.

elasticity of steam destroys its special centripetal tendency, relatively to the earth, without materially affecting the solar influence on its orbital motion; and its volume is increased nearly in inverse ratio to the Sun's local attraction.

Ganot[*] gives 1698 : 1 as the volume-ratio of steam at 100° C. to water at 0° C., and he estimates the expansion of water between 0° and 100°[†] at $\cdot 0466 + \cdot 002584 = \cdot 049184$. A nucleal expansion of $1\cdot 049184$ corresponds to a nebular expansion of $1\cdot 049184^{\frac{4}{3}} = 1\cdot 066$; and $1698 \div 1\cdot 066 = 1593$, or very nearly n^3 ($= 1584$). If Sun's mean distance is 93,000,000 miles, the mean ratio of its attractive force upon Earth to Earth's equatorial gravitation is 1665.

LI. *On the Difference of Potential produced by the Contact of different Substances.* By Professor R. B. CLIFTON.

To the Editors of the Philosophical Magazine and Journal.

GENTLEMEN,

IN the March Number of the Philosophical Magazine is published a letter from Professors Ayrton and Perry, in which they refer to my paper "On the Difference of Potential produced by the Contact of different Substances" (Proceedings of the Royal Society, vol. xxvi.). To this letter I have hitherto been prevented from making a reply; but I now venture to ask for the insertion in your Journal of a few remarks on some of the matters therein referred to.

At the time my paper was written I was quite unaware of the investigation of the same subject which had been undertaken by Professors Ayrton and Perry; and, indeed, the first information I obtained relative to their work was derived from the letter above mentioned. As soon as possible after the appearance of their communication I endeavoured to find their paper, by searching through the various scientific periodicals and the publications of scientific societies, but without success. The title of the paper appeared in the 'Report of the British Association,' but the title only.

As I was not present at the Meeting of the British Association at Glasgow, at which some account of the work of Professors Ayrton and Perry appears to have been given, but not reported, my ignorance of their work is, I hope, excusable, though I very much regret it. The paper in question has recently been read before the Royal Society, and it will, I believe, shortly be published.

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* Fifth Engl. ed., § 353.　　† Ibid. §§ 294–5.

The unsatisfactory nature of the explanation of voltaic action given in modern treatises on electricity has induced Professors Ayrton and Perry to undertake the investigation to which they draw attention in their letter; and the same circumstance led me to give special attention to the subject; we even refer to the same passage in the textbook by Professor Fleeming Jenkin as an instance of statements requiring further explanation. It is therefore not extraordinary that there should be a great resemblance between the series of metals and liquids selected for examination by the two Professors and by myself; but as they were working perfectly independent of me, and I of them, the claim of priority seems to be devoid of meaning.

I shall not attempt to discuss the question of priority; it is a matter in which I do not feel the slightest interest; but as Professors Ayrton and Perry state that I appear to have commenced my earliest experiments on the subject several months after the Meeting of the British Association at Glasgow in 1876, I think it better, in order to avoid any difficulty in the future, to mention that I began to give special attention to this subject in 1874; and although frequent interruptions prevented me from making much progress, all the principal results communicated in my paper were obtained before the summer of 1876, and were introduced into my lectures delivered during Michaelmas Term 1876, and Hilary Term 1877. In my paper I have only given some of the *latest* quantitative determinations which I made—partly because I considered them the best, but more especially because I had, in making them, employed Clark's standard cell, to the variation of which I wished to draw attention. In my earlier experiments I had used a Daniell's cell.

The method of investigating the difference of potential arising from the contact of different substances, employed by Professors Ayrton and Perry, is quite distinct from that which I adopted, so far at least as I am able to judge from the description without the drawing, which I have not yet seen.

Their method, which seems to me both ingenious and novel, possesses the great advantage of permitting two liquids to be treated in the same way as two solids, or as a solid and a liquid. The method which I adopted is essentially the same as that employed by Kohlrausch in the case of two metals; but the condenser was furnished with horizontal plates, for one of which a vessel of liquid could be substituted. The only changes which I introduced consist in the arrangements for adjusting and moving the opposed plates, in the mode of insulating, and in the form of key used with the condenser.

My apparatus enabled me not only to observe the sign of the difference of potential due to metal-liquid contacts, but also to obtain quantitative determinations ; as, however, some of these differences of potential are very small, and a slight error in the construction of the condenser, which I believed to exist, would probably produce considerable errors in the values obtained for the potentials, I thought it best not to introduce quantitative results in which I felt little confidence into my paper, which I only regarded as a preliminary notice.

Between most of the results obtained by Professors Ayrton and Perry and those published by myself, there is a fairly satisfactory agreement from a qualitative point of view ; but between such quantitative measures as admit of comparison there seem to be considerable descrepancies : these I trust will disappear when we introduce into our respective methods the improvements which we each admit to be necessary.

Several of the criticisms on my paper, which have been introduced into the letter of Professors Ayrton and Perry, do not appear to me well founded ; but I must for want of time postpone the discussion of them ; and possibly the more thorough investigation of the subject, which we each contemplate, may render this discussion unnecessary.

As the value of any results we may obtain in the future would be much increased by a ready means of comparison, I venture to suggest that we should adopt the same difference of potential as the unit. If I am right as to the variation in the difference of potential exhibited by the terminals of Clark's standard cell, it will clearly be undesirable to employ this cell; and I should suggest the use of the standard Daniell's cell referred to in my paper, viz. copper in a saturated solution of copper sulphate, and amalgamated zinc in a mixture of one part by weight of pure sulphuric acid and four parts by weight of distilled water. This cell seems to me to give a remarkably constant difference of potential when prepared on different occasions ; and the accurate expression of this difference, in volts, when obtained, would at once allow all our results to be translated into terms of the latter unit.

I am, Gentlemen,

Yours faithfully,

R. B. Clifton.

Oxford, April 22, 1878.

LII. *Problems relating to Underground Temperature. A Fragment. By* Sir W. Thomson[*].

*P*ROBLEM I.—A fire is lighted on a small portion of an uninterrupted plane boundary of a mass of rock of the precise quality of that of Calton Hill, and after burning for a certain time is removed, the whole plane area of rock being then freely exposed to the atmosphere. It is required to determine the consequent conduction of heat through the interior.

Problem II.—It is required to trace the effect of an unusually hot day on the internal temperature of such a mass of rock.

Problem III.—It is required to trace the secular effect consequent on a sudden alteration of mean temperature.

Problem IV.—It is required to determine the change of temperature within a ball of the rock consequent upon suddenly removing it from a fluid of one constant temperature and plunging it into a fluid maintained at another constant temperature.

Problems I., II., and III. In solving each of these problems, we shall suppose the air in contact with the rock to be not sensibly influenced in its temperature by the conduction of heat inwards or outwards through the solid substance. In reality, the stratum of air in immediate contact with the rock must always have precisely the same temperature as the rock itself at its bounding surface; and the continual mixing up of the different strata, whether by wind or by local convective currents due to differences of temperature, tends to bring the whole superincumbent mass of air to one temperature. Our supposition therefore amounts to assuming that the rate of variation of temperature from point to point in the rock near its surface, owing to the special cause under consideration, is much less than the ordinary changing variations from day to night. Hence, in Problems I., II., and III., the solutions will not be applicable until so much time has been allowed to elapse as will leave only a residual variation, small in comparison with

* Communicated by the Author. An old MS., written eighteen years ago and found today. It was kept back until the time should be found to write out the solutions of Problems II., III., and IV. The time was never found; but as mere synthesis from the solution of Problem I. suffices for II. and III. (surface integration of the solution for I. over the medial plane solves II., and the time-integral from $t = -\infty$ to $t=0$, of the solution of II., gives that of III.), and as IV. is merely an example of Fourier's now well-known solution for the globe (see Professors Ayrton and Perry's paper, "On the Heat-conductivity of Stone," Philosophical Magazine for April 1878), with numerical results calculated for trap-rock according to its thermal conductivity, as determined by the Edinburgh observations referred to in the fragment now published, the non-completion of the original proposal need not be much regretted.—W. T., March 25, 1878.

the ordinary diurnal maximum rates of increase and diminution of temperature from point to point inwards in the immediate neighbourhood of the surface.

In the case of Problem II. these conditions will be practically fulfilled, and continue to be fulfilled, very soon after the day of extraordinary temperature of which the effect is to be considered, and we shall have a perfectly practical solution illustrative of the consequences experienced several days or weeks later at the 3-foot and 6-foot deep thermometers of the observing-station. The solution of Problem I., which we now proceed to work out, will show clearly what dimensions as to space, time, and temperature may be chosen for a really practical illustration of its conclusions.

Problem I., subject to the limitations we have just stated, is equivalent to the following:—*An infinitely small area of an infinite plane terminating on one side a mass of uniform traprock which extends up indefinitely in all directions on the other side, is infinitely heated for an infinitely short time, and the whole surface is instantly and for ever after maintained at a constant temperature. It is required to determine the consequent internal variations of temperature.*

Let the solid be doubled so as to extend to an infinite distance on both sides of the plane mentioned in the enunciation. This plane, when no longer a boundary, we shall call *the medial plane*. Let P, P′ be two points equidistant from the medial plane in a line perpendicular to it, on each side of the portion heated according to enunciation. Let a certain quantity of heat Q be suddenly created in an infinitely small portion of the solid round P, and at the same instant let an equal quantity be abstracted from an infinitely small portion of the solid round P′. The consequent variations of temperature on the two sides of the medial plane of reference will be equal and opposite, being a heating effect which spreads from the medial plane in one direction, and a symmetrical cooling effect spreading from the same plane through the matter which we have imagined placed on its other side. The heating effect on the first side will, as is easily seen, be precisely the same as that proposed for investigation in Problem I.; and the thermal action of the mass we have supposed added on the other side will merely have the effect of maintaining the temperature of the bounding plane unvaried. Now if a quantity Q of heat be placed at one point (α, β, γ) of an infinite homogeneous solid, the effect at any subsequent time t at any point x, y, z of the solid will be expressed by the formula

$$\frac{Q}{8\sqrt{k^3\pi^3}} \cdot t^{-\frac{3}{2}} \epsilon^{-\frac{(x-\alpha)^2+(y-\beta)^2+(z-\gamma)^2}{4kt}},$$

discovered by Fourier : and the effect of simultaneously pla-
cing other quantities of heat, positive or negative, at other
points will, as he has shown, be determined by finding the
effect of each source separately by proper application of the
same formula, and adding the results in accordance with the
principle of the superposition of thermal conductions stated
above.   Hence the effect of simultaneously placing equal po-
sitive and negative quantities, $+Q$ and $-Q$, at two points,
(α, β, γ), $(\alpha', \beta', \gamma')$, will for any subsequent time t be ex-
pressed by the formula

$$\frac{Q}{8\sqrt{k^3\pi^3}}\, t^{-\frac{3}{2}}\left\{\epsilon^{-\frac{(x-\alpha)^2+(y-\beta)^2+(z-\gamma)^2}{4kt}} - \epsilon^{-\frac{(x-\alpha')^2+(y-\beta')^2+(z-\gamma')^2}{4kt}}\right\}.$$

If in this expression we take $\alpha = \frac{1}{2}a$, $\alpha' = -\frac{1}{2}a$, $\beta = 0$, $\beta' = 0$,
$\gamma = 0$, $\gamma' = 0$, and suppose a to be infinitely small, we
find what it becomes by differentiating the first term with
reference to α, writing a instead of $d\alpha$, and taking $\alpha = 0$, $\beta = 0$,
$\gamma = 0$. The result constitutes the solution of the proposed
problem ; and thus, if v denote the required temperature at
time t and point (x, y, z) of the solid, we find

$$v = \frac{Qa}{16\pi^{\frac{3}{2}}k^{\frac{5}{2}}}\,.\,xt^{-\frac{5}{2}}\epsilon^{-\frac{x^2+y^2+z^2}{4kt}}.$$

A more convenient formula* to express the solution will be

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* In this formula k denotes what I have called the thermal diffusivity
of the substance—that is to say, its thermal conductivity divided by the
thermal capacity of unit bulk of the substance.   Diffusivity is essentially
reckoned in units of area per unit of time ; or, as Maxwell puts it, its di-
mensions are $\left[\dfrac{L^2}{T}\right]$.   Its value (141 square British feet per annum for the
trap-rock of Calton Hill, used further on in the text) was taken from my
paper on the " Reduction of Observations of Underground Temperature,"
published in the Transactions of the Royal Society of Edinburgh for
April 1860, where it was found by the application of Fourier's original
formula to a harmonic reduction of Forbes's observations of underground
temperature.   Reducing this number to square centimetres per second,
and expressing similarly the results of my own reduction of Forbes's ob-
servations for two other localities in the neighbourhood of Edinburgh,
and of Professor Everett's reductions of the Greenwich Underground
Observations, we have the following Table of diffusivities :—

<div align="center">Diffusivities.</div>

Trap-rock of Calton Hill . . . . . . ·00786	of a square centim. per second.		
Sand of experimental garden . . . . ·00872	,,	,,	,,
Sandstone of Craigleith Quarry. . ·02311	,,	,,	,,
Gravel of Greenwich Observatory } ·01249 Hill . . . . }	,,	,,	,,

These numbers were first published by Everett, in his 'Illustrations
of the Centimetre-Gramme-Second (C. G. S) System of Units,' published
by the Physical Society of London (1875), a most opportune and useful
publication.

obtained by putting $x^2 + y^2 + z^2 = r^2$ and $x = r \cos \theta$. We thus have

$$v = \frac{Qa}{16\pi^{\frac{3}{2}}} (kt)^{-\frac{5}{2}} \cos \theta \cdot r\epsilon^{-\frac{r^2}{4kt}},$$

which expresses the temperature assumed at a time t after the application of the fire, by a point of the solid at a distance r from the point of the surface where the fire was applied, and situated in a direction inclined at an angle θ to the vertical through this point. From this expression we conclude:—

(1) The simultaneous temperatures at different points equidistant from the position of the fire are simply proportional to the distances of these points from the plane surface.

(2) The law of variation of temperature with distance in any one line from the place where the fire was applied is the same at all times.

(3) The law of variation of temperature with time is the same at all points of the solid.

(4) Corresponding distances in the law of variation with distance increase in proportion to the square root of the time from the application and removal of the fire; and therefore, of course, corresponding times in the law of variation with time are proportional to the squares of the distances.

(5) The maximum value of the temperature, in the law of variation with distance, diminishes inversely as the square of the increasing time.

(6) The maximum value of the temperature in the law of variation with time, at any one point of the rock, is inversely as the fourth power of the distance from the place where the fire was applied.

(7) At any one time subsequent to the application of the fire, the temperature increases in any direction from the place where the fire was applied to a maximum at a distance equal to $\sqrt{2kt}$, and beyond that falls to zero at an infinite distance in every direction. The value of k for the trap-rock of Calton Hill being 141, when a year is taken as the unit of time and a British foot the unit of space, the radius of the hemispherical surface of maximum temperature is therefore $16\cdot8 \times \sqrt{t}$ feet. Thus at the end of one year it is $16\cdot8$ feet, at the end of 10,000 years it is 1689 feet, from the origin. The curve of fig. 1 shows graphically the law of variation of temperature with distance. The ordinates of the curve are proportional to the temperatures, and the corresponding abscissas to the distances from the origin or place of application of the fire.

(8) At any one point at a finite distance within the solid (which, by hypothesis, is at temperature zero at the instant when

the fire is applied and removed) the temperature increases to a maximum at a certain time, and then diminishes to zero again

Fig. 1.
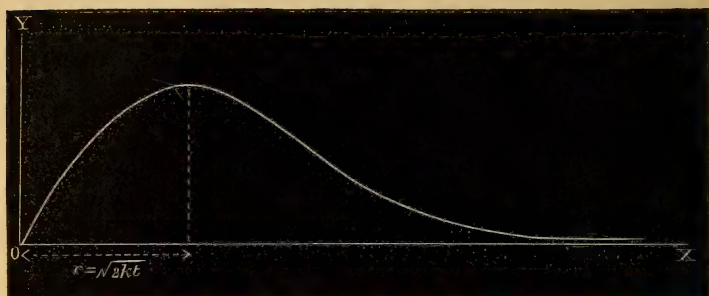
after an infinite time; the ultimate law of diminution being inversely as the square root of the fifth power of the time. The time when the maximum temperature is acquired at a distance r from the place where the fire was applied, is $\dfrac{r^2}{10k}$, or, according to the value we have found for the trap-rock, $\dfrac{r^2}{237\cdot5}$ of a year. Thus it appears that at one French foot from the place of the fire, the maximum temperature is acquired a day and a half (more exactly 1·54 day) after the application and removal of the fire. At 15·4 French feet from the fire the maximum temperature is reached just a year from the beginning; and at 1540 feet the maximum is reached in 10,000 years. The law of variation of temperature with time is shown by the curve of fig. 2, the ordinates of which represent temperatures, and the abscissas times.

Fig. 2.
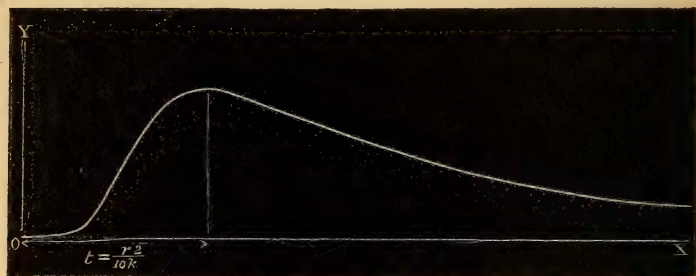
From these results we can readily see how the circumstances of the proposed problem may be actually realized, if not rigorously, yet to any desired degree of approximation, in the manner supposed—namely, by keeping a fire burning for a certain time over a small area of rock, and then removing it and cooling the surface.

LIII. *On the Electric Conductivity and Electrolysis of Chemical Compounds.* By Dr. L. BLEEKRODE[*].

HITTORF states, as a result of his investigations on electrolysis[†], that electrolytes are characterized by corresponding ions in them being capable of replacing one another. They are consequently salts in the sense attached to the term by modern chemistry; and during electrolysis exchange takes place between the same constituents of their molecules as in double elective affinity. With the difficulty of this exchange he connects the resistance of the electrolytes to electrical conduction; so that on this account water, for example, and prussic acid are such bad conductors. In Magnus's theory[‡] the replaceability of hydrogen by other substances is especially taken into consideration. It therefore appeared to me desirable both to test the consequences deduced from Hittorf's experiments and to try whether the presence of hydrogen replaceable in the compound either by metals or atomic groups (radicals) is connected with capability of electrolysis. If so, the latter should be absent when the above-mentioned chemical exchange does not take place. I selected the simplest compounds which could be preserved liquid without a solvent: to this category belong, of course, the condensed gases and also a great number of bodies derived from organic chemistry, as, for instance, the organo-metallic radicals, the substitution-products of ammonia, &c. Up to the present time very few organic substances have been thus investigated with respect to their electrolyzability; and therefore I thought it would not be unimportant to supply this deficiency.

1. *Arrangement of the Experiments.*

The material available for these experiments was rather limited, as I had to select compounds containing either a metal or hydrogen, and which could also be kept in the liquid state, whether directly or by high pressure, as the gases, or by raising their temperature. For the condensed gases it was most convenient to employ Faraday's method. They were therefore enclosed in stout tubes of glass, into the two ends of which annealed platinum wires were fused to serve as electrodes. One of these traversed the entire length of the tube to within 1, 2, or 3 millims. of the other. They were separated by a layer of the liquid; and by this arrangement conduction along the glass was almost entirely prevented. The other[*] substances,

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not needing any high pressure to liquefy them, were enclosed in short straight tubes; so that small quantities sufficed. The conductivity of the electrolyzed substances was estimated from the deflections of the needle of a galvanometer the coil of which was of thin wire, and in which the current from a couple of wires, zinc and silver, immersed in distilled water gave a deflection of 5°, and of two silver wires in water, which were inserted in the circuit, a deflection of 2°. The current for the experiments on electrolysis was generated by:—first, a galvanic battery of twenty large Bunsen-Deleuil elements; secondly, a battery of forty elements, which liberated in the voltameter 600 cubic centims. of oxyhydrogen gas per minute; and, thirdly, a battery of eighty elements, which produced 840 cubic centims. per minute. Other series of experiments were made with induction-currents: for these I used a Ruhmkorff's induction-coil with a spark-length of 15 millims., and a second, the spark of which attained a length 42–70 millims.

No currents of such intensity have, to my knowledge, been employed for electrolytic purposes, especially with the substances investigated by me, except in the experiments of Lapschin and Tichanowitsch *. Warren De la Rue's chloride-of-silver battery furnishes currents of far greater intensity; my experiments with it shall be discussed in a separate section.

The current was led through the liquid and at the same time through the galvanometer; but it might be questioned whether the deflection was not conditioned by conduction of the current in the glass tube †, and by a metallic, not an electrolytic conduction. In order to determine what was the amount (if any) of conduction along the glass, if the galvanometer showed only a slight deflection, the liquid was removed from between the electrodes by inverting the tube; the circuit could then be closed only by the glass sides. The deflection now produced I have named the glass-conduction. As, with the exception of alloys, no experiments had shown that compound substances conduct in metallic fashion only, and not at the same time electrolytically, I thought I could infer from the deflection of the galvanometer, not only the conductivity of the compound, but also its liability to electrolysis, although for the most part I could not observe any polarization-current.

2. *Electrolysis of Water.*

All the experiments which have been made, and especially the more recent observations of Kohlrausch, prove that water

* *Bull. de l'Acad. de St. Pétersb.* vol. iv. 1861; Phil. Mag. [IV.] vol. xxii. p. 308.
 † Conf. Beetz, Pogg. *Ann.* vol. xcii. p. 465.

and alcohol, when as pure as possible, are almost non-conductors of the current. Lapschin and Tichanowitsch also found that alcohol did not conduct the current of 1000 elements unless it had absorbed moisture.

These two bodies, which at all events belong to the class of bad conductors, show that even the presence of hydrogen which is readily interchanged with metals does not always imply a sufficient conduction for electrolysis. In both we can make a direct substitution of potassium and sodium for hydrogen; and in water we can replace the hydrogen by iron; and yet this property does not seem in this case to possess any special significance for the conduction. Even elective affinity appears here to take no part therein; otherwise the electrolysis of these substances could not be so doubtful, since several reactions are known in which they exchange their constituents —*e. g.* water with oxide of potassium, water with alcoholates*, consequently in both cases with electrolytes. With alcohol, however, decomposition by the metallic chlorides, for example, requires the application of heat.

3. *Electrolysis of liquefied Hydrogen Acids.*

The combination of hydrogen with the metalloids (chlorine, bromine, iodine, sulphur, &c.) furnishes bodies the electrolysis of which was specially important for the purpose of this investigation, since they exhibit numerous chemical reactions, and nearly all of them had only been tested hitherto, as to their electrolyzability, dissolved in water. As they are all gaseous, it was necessary to condense them.

Faraday's method proved here the most suitable; it gives with tolerable facility a sufficient quantity of great purity. The process of condensing the hydride of chlorine was as follows:—After a platinum wire had been fused into one end of a strong glass tube, reaching to the other extremity, concentrated sulphuric acid was poured in till the tube was half filled, upon which, separated by a small disk of blotting-paper, was placed a layer of crystals of chloride of ammonium. Further, the second extremity of the tube was bent round, and into it a short platinum wire was inserted to serve as the second electrode, and, the tube being closed by fusing, fixed at a distance of 2 or 3 millims. from the first wire. The two

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* In this case the reaction is:—

$$\left.\begin{matrix} C_2H_5 \\ K \end{matrix}\right\}O + \left.\begin{matrix} H \\ H \end{matrix}\right\}O = \left.\begin{matrix} C_2H_5 \\ H \end{matrix}\right\}O + \left.\begin{matrix} H \\ K \end{matrix}\right\}O.$$
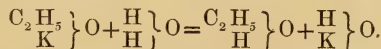
This alcoholate is an electrolyte; with the battery of 20 Bunsen's elements a rapid evolution of gas took place, while the liquid became brown-coloured.

wires were consequently never connected with the same part of the tube, so that conduction along the glass was rendered difficult.   By agitation and suitable motion the substances for the production of the gas were now brought into contact; and the action was assisted by moderate heating; special care was necessary to prevent the products of decomposition from being brought over by the gas.   In all the experiments I finally succeeded in collecting in the bent part of the tube a quantity of perfectly pure liquefied gas.   The liquid sulphide of hydrogen was prepared from sulphide of iron and dilute sulphuric acid; hydriodic acid from a mixture of iodine, iodide of potassium, and moistened red phosphorus; hydrobromic acid from liquid bromine and a mixture of bromide of potassium and moistened red phosphorus; arseniuretted hydrogen from arsenide of zinc and diluted sulphuric acid; hydrocyanic acid from cyanide of mercury by decomposition with sulphuric acid and cooling.   A small quantity of chloride of calcium or pure lime was introduced into the tube, near the bent portion, to dry the gas as it was formed.   The results of the experiments with these substances are contained in the following Table, in which D signifies the deflection of the galvanometer-needle on the passage of the current through the liquid, and G the deflection corresponding to the conduction along the glass :—

TABLE I.

Compound.	Galvanic battery.		Induction-coil.		Remarks.
	20 Bunsen's elements.	40 Bunsen's elements.	Spark-disch. = 15 millims.	Spark-disch. = 70 millims	
HCl......	D = 0°	G = 0° D = 0	D = 4°*	D = 10°	Spark-discharge in the liquid, and the tube shattered.
HBr ...	D = 3	G = 3 D = 3	D = 0	D = 8	Spark-discharge, and the tube shattered.
HI	D = 0	G = 0 D = 0	D = 0	D = 6	Heat evolved and iodine separated.
H₂S......	D = 0	G = 0 D = 0	D = 0	D = 6	Spark-discharge, considerable evolution of heat, tube shattered.
H₃As ...	......	G = 0 D = 0	D = 0†	......	No discharge; distance between the electrodes = 3 millims.
HFl...... HCN ...	Trace. D = 90	D = 90*	D = 3‡	......	Violet-coloured sparks.

    * Rapid evolution of gas at the electrodes.
    † Undulatory motion at the surface.
    ‡ Spark-discharge and production of heat.

It follows from these experiments that most of the hydrogen acids are extraordinarily bad conductors of the galvanic current. This behaviour is the more remarkable as the compounds here present are, when dissolved in water (likewise a very bad conductor), some of the best conductors and electrolytes. For this very reason, because a slight trace of water is sufficient to much increase the conductivity, the negative result may be regarded as a proof of the adequate purity of the condensed gases[*]. The only exception is hydrocyanic acid, it being decomposed by the current of only 3 or 5 small Bunsen elements. I endeavoured to condense the gas perfectly dry by decomposing dry cyanide of mercury with dry sulphuretted hydrogen and conducting the gas formed over phosphoric anhydride before it entered the condensation-tube. At the negative pole hydrogen was evolved; and at the positive pole platinum cyanide was probably formed, which coloured the liquid yellow. Moreover this compound is readily decomposed chemically; it is even decomposed by light, with formation of ammonia.

These bodies retain their insulating properties when submitted to electricity of high tension; for only the more powerful spark-discharges can take place in them when the distance between the electrodes is from 2 to 4 millims. The conduction of induction-currents is in this case complicated, since it is difficult to decide whether a merely mechanical discharge takes place, occasioning, by the production of heat, a sort of dissociation of the compound, or whether it is accompanied by a proper electrolytic action.

With hydrochloric acid[†], even when moderate induction-currents were employed, an effervescence was observed at the electrodes. In order to determine the cause of this, the induction-currents (spark-length 15 millims. in air) were directed through the liquid acid during four hours; but no spark-discharge took place. The part of the tube containing

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[*] This influence of slight admixtures is evident, for instance, from an observation made by Becquerel, who was able to decompose alcohol with a battery of 50 elements if only $\frac{1}{1000}$ of potassium was added. Despretz found the electrolysis of water with $\frac{1}{2000}$ of sulphuric acid different from that of distilled water. The same fact has also been pointed out by Kohlrausch (Pogg. *Ann.* Ergzbd. viii. p. 4).

[†] The conductivity of chloride and fluoride of hydrogen in the liquid state has also been investigated by Gore (Proc. Royal Soc. vol. xvii. p. 256). With a pile of 10 Smee's elements and with the electrodes separated by a distance of 0·16 millim. the galvanometer showed a small deflection; likewise with the induction-coil conduction was perceptible accompanied by chemical action. Gore, however, thinks that the liquefied gas was not perfectly pure. Hydrofluoric acid, with gold electrodes, exhibited almost no conduction.

the acid was placed in a freezing-mixture, by which the temperature was kept down to 0° C. at least.   At the close of the experiment no alteration could be perceived on the surface of the platinum electrodes.   On very strongly re-frigerating the tube no effervescence could any longer be observed, but instead an undulatory motion of the surface : this phenomenon will be considered in a subsequent part of this paper.   The tube being now taken from the freezing-mixture and exposed to the temperature of the air, the effer-vescence soon reappeared at the electrodes.   This shows that the gas-evolution was only a consequence of the heating of the electrodes by the discharges.   No polarization was afterwards manifested by the galvanometer ; nevertheless the liquid may have been so far conducting that the current passed through at the opening and closing of the circuit ; or the resistance of the liquid may have been too great to permit a polarization-current to produce a deflection.

As the conductivity of these bodies when liquefied is quite different from their conductivity when in the state of solution, so is their chemical character very different.   I have been able to demonstrate this with respect to hydrochloric acid, by placing in the condensation-tube, before closing the circuit, a white strip of zinc and then condensing the gas, so that the metal was completely immersed in the liquid.   Only at the end of seven weeks could small white spots of chloride of zinc be here and there perceived on the strip; everywhere else the metallic appearance was pretty well preserved.   After nearly a year a slight white precipitate could be seen in the tube.   On opening the tube the strip of zinc, exposed to the air, immediately became moist from the hygroscopic property of the coating of chloride of zinc.   On a strip of copper it was not till after four months that a little white powder was visible ; and subsequently a white precipitate gradually formed in the tube, most probably of anhydrous protochloride of copper *.   In liquefied sulphuretted hydrogen a strip of zinc was first distinctly attacked after fourteen days, with the formation of a white precipitate (sulphide of zinc) at the bottom of the tube.   Wires of silver and copper were already, before the condensation of the gaseous acid, coated with a sulphide, which then protected them from further attack. Accordingly the liquefied hydrogen acids exchange their hydrogen with great difficulty for the metals with which, when dissolved in water, they at once form metallic chlorides &c.   It

* For similar experiments by Gore with strips of lead, iron, and magnesium, see Phil. Mag. [IV.] vol. xxix. p. 547, and Proc. Roy. Soc. vol. xvii.

might, perhaps, be thought that this difficulty resulted from the conditions of the experiment itself, the first-formed layer of metallic chloride or the separated hydrogen adhering to the metal in consequence of the high pressure (50–60 atmospheres) and acting as a protecting film ; but Gore (*l. c.*) has established other evidences of a feebler chemical activity—viz. that liquid hydrochloric acid has no action on quicklime, acts but very slowly on metallic oxides and carbonates, with the exception of carbonate of calcium, and that liquid hydrofluoric acid acts only on potassium and sodium, not on other metals, producing a brisk liberation of hydrogen [*].

Lastly, the extremely slight solubility of the newly formed substances in the liquid acids might, perhaps, have some influence ; they appear, even when dissolved, to diffuse very slowly, and finally to settle at the bottom of the tube. In favour of this idea may be alleged an experiment of mine, in which calcium carbonate was decomposed by ordinary diluted hydrochloric acid, even under a pressure of 50–60 atmospheres ; here the water could immediately act as a solvent on the calcium chloride formed. It is therefore very doubtful if the difficulty of exchange of the hydrogen in the liquefied gases depends on their special chemical character ; and hence there is no substantial reason for connecting their feeble conductivity with it. Besides, it is proved that a direct action of the high pressure cannot have occasioned both phenomena. The experiments of Favre, Berthelot, and Cailletet have shown that even a pressure of 300 atmospheres does not directly interfere with the chemical phenomena ; and the electrolysis of water was observed by Gassiot [†] to take place even under a pressure of 86 atmospheres.

No important alteration was found by Herwig, recently, in the conductivity of a solution of cupric sulphate under the pressure of 18 atmospheres ; consequently there are no grounds for attributing to the pressure in these cases any direct influence on the results.

4. *Electrolysis of Anhydrides.*

As a number of anhydrides possess no hydrogen in their composition, they ought, according to the preceding hypotheses, to belong to the class of bad conductors. To test this I selected only those which could be obtained in a high state of purity and were fusible without decomposition. Hence

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[*] With the hydrofluoric acid the pressure was not above one atmosphere.

[†] *Jahresb. für Chemie*, 1854, p. 523.

the material was limited. The nitric and phosphoric anhydrides could not be employed ; and only two gases, sulphurous and carbonic acids, required to be condensed. The former was liquefied by means of a freezing-mixture, the latter by Faraday's method with bicarbonate of ammonia and dilute sulphuric acid. Table II. comprises the results.

TABLE II.

Anhydrides.	Galvanic battery.		Induction-coil.		Remarks.
	20 Bunsen elements.	80 Bunsen elements.	Spark-l. 15 mm.	Spark-l. 70 mm.	
Sulphurous, SO_2	Trace of defl.	Trace of defl.	$D = \overset{\circ}{0}$	$D = 1\overset{\circ}{0}$	No spark-discharge, effervescence at the electrodes.
Sulphuric, SO_3	$D = 0$	$D = 8$ $G = 6$	......	$D = 90$	Brisk effervescence at the electrodes.
Carbonic, CO_2	$D = 0$	$D = 0$ $G = 0$	Trace of defl.	Spark-disch.	Tube shattered by the discharge.
Boracic, Bo_2O_3	$D = 0$	......	......	......	No conduction of the current from 950 Bunsen elements (L. & T.).
Arsenious, As_2O_3	$D = 0$	$D = 90$	......	$D = 10$	
Chromic, CrO_3	Conductor.	$D = 0$	......	$D = 40$	A black residuum separated with the discharge.
Osmic, OsO_4	$D = 0$			Decomposition.	
Vanadic, VO_3	$D = 3\overset{\circ}{0}$	......	......	......	Decomposition and gas evolved by the current.
Molybdic, MoO_3	Conductor.	......	......	......	,, ,, ,,
Acetic, $(C_2H_3O)_2O$	$D = 25$	$D = 40$	$D = 4$ Gas liberated at electr.	$D = 6$	Brisk evolution of gas and polarization of the electrodes by the galvanic current.
Benzoic, $(C_7H_5O)_2O$	$D = 3$	......	Spark-disch.	......	With 1 millim. distance between the electrodes, sparks and deposition of carbon.
Phthalic, $C_6H_4(CO)_2O$	$D = 20$	......	$D = 6$ No spark.	......	With the induction-coil rapid decomposition and gas liberated at both electrodes.
Malic, $(C_2HO)_2O$	$D = 8$	......	Decomp., no spark.	......	Distance of electrodes = 3 millims.
Butyric, $(C_4H_7O)_2O$	$D = 3$	......	Spark-disch.	......	Sparks blue. Distance of electrodes = $1\frac{1}{2}$ millim.

This Table embraces three groups of anhydrides—namely, the anhydrides in which the oxygen is combined with a metalloid, the anhydrides of the organic acids, and the anhydrides containing metals. The first group includes very bad conductors: with the sulphurous and sulphuric anhydrides the conduction is hardly perceptible—deflection, with 80 Bunsen

elements, only 2°; and this may arise from a trace of water, since the preparation of the first compound was effected by a freezing-mixture, and the second, although it forms a well-crystallized substance, is extremely hygroscopic : it is therefore with difficulty protected from the aqueous vapour of the air. This is more successfully accomplished with carbonic anhydride, which then exhibits no conduction at all [*].

Of the chemical reactions of these bodies little is known. With the aid of a little heat, various metals act reducingly on them. Gore says that liquefied carbonic anhydride exerts no chemical action, has but very slight solvent power, and is reduced only by the alkali-metals.

The anhydrides which contain metals belong (so far as is known) to the good conductors, with the exception of osmic anhydride. This compound, which fuses at 40° C. is not decomposed by a very powerful galvanic current (80° B. e.), although its chemical properties are similar to those of the three other anhydrides, which were split up by a battery of 16 Bunsen elements [†]; and it parts with its oxygen as readily as these do to oxidable substances. In a similar manner it is seen that facility of chemical decomposition is not accompanied by high conductivity in hyponitric acid (NO_2), which, however, is not properly an anhydride, and therefore is not included in the above Table. This gas was liquefied by a freezing-mixture, and was perfectly pure. In this condition it was completely non-conducting to the current from 80 Bunsen elements ; and with the electrodes only one millim. apart an induction spark of 15 millims. length would not pass. Yet, according to Hittorf's theory, this compound should be an electrolyte, since with water or hydrate of potassium it suffers decomposition by elective affinity. In the liquid state it acts on metals and metallic oxides, with which it forms nitrates, at the same time liberating nitrous oxide [‡]. As in the case of hydrofluoric acid, so here the pressure was only that of the atmosphere, and the metals (as lead, mercury, potassium, and sodium) reacted at a very low temperature, although somewhat slowly. The chemical properties appear therefore not to be much modified by the liquid state alone.

The anhydrides of the organic acids are, within the limits

[*] Gore, Phil. Trans. 1861, pt. i. p. 83. When the electrodes were 2 millims. apart the current of 40 Smee elements did not pass through ; and just as little did the discharge of an induction-coil when only 0·2 millim. of liquid separated the electrodes.

[†] Their electrolysis has been effected by Buff: see Pogg. *Ann.* vol. cx. p. 267.

[‡] Richard Müller, Liebig's *Annalen,* vol. cxxii. p. 1 (1859).

of my experiments, conductors, although it cannot be maintained that facility of chemical decomposition is connected therewith. In acetic anhydride the hydrogen is expelled by potassium and by zinc; the corresponding decomposition of other compounds, however, takes place only at higher temperatures: in benzoic and phthalic anhydrides the entire molecule often passes without decomposition into the new compound.

5. *Electrolysis of Amines and Amides (Ammonia-substitutes).*

The first of these I examined was ammonia itself. The chlorides of silver and calcium were saturated with the gas; and with them quicklime and sodium were enclosed in the condensation-tube, in order, on expelling the gas by heating, to remove the last traces of water. The separated liquid ammonia was several times poured back over the sodium by inverting the tube, and redistilled*.

Liquid ammonia is a good conductor, and is decomposed by the current. Twenty Bunsen elements gave a deflection of from 5° to 10°, which with 80 elements rose to 90°. Thereupon was seen the peculiar phenomenon of the liquid becoming of a bright blue colour simultaneously with an abundant liberation of gas. This colour disappeared as soon as the current ceased; and the liquid became hot. Ammonia, then, behaves as an electrolyte. The positive electrode was blackened; and a polarization-current was afterwards very perceptible. It was not possible to decide definitely respecting the decomposition-products; a splitting into H and NH_2 is possible, although NH_2 is only known in combination with metals †. The results with substances analogous to ammonia are given in Table III.

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* I have also tried condensing it with liquid sulphurous acid; but then contamination with aqueous vapour from the air and the formation of sulphite of ammonia were more to be feared.

† In Pogg. *Ann.* vol. cxxi. p. 607, Weyl describes the formation of the body $\left.{NH_3 \atop H}\right\}$. He obtained a blue fluid which separated into its elements at a temperature of from 12° to 14° C. The commencement of decomposition is shown by the previously deep-dark-blue liquid beginning to lose its colour; and finally perfectly colourless ammonia is left behind as a very mobile liquid. This agrees with my observations; therefore in these also ammonia may have been separated, according to the reaction

$$2NH_3 = NH_4 + NH_2.$$

TABLE III.

Compound.	Galvanic battery.		Induction-coil.	
	20 B. el.	80 B. el.	Spark-length =15 millims.	Spark-length =70 millims.
Ammonia[1], $\left.\begin{array}{c} H \\ H \\ H \end{array}\right\} N$	G=0̊ D=5	G=0̊ D=5	D=10̊ Brisk effervescence.	D=20̊ No spark-discharge.
Ethylamine[2], $\left.\begin{array}{c} C_2H_5 \\ H \\ H \end{array}\right\} N$	D=4	D=20	D=4	Strong heating.
Diethylamine[3], $\left.\begin{array}{c} C_2H_5 \\ C_2H_5 \\ H \end{array}\right\} N$	D=3	....	....	
Triethylamine[4], $\left.\begin{array}{c} C_2H_5 \\ C_2H_5 \\ C_2H_5 \end{array}\right\} N$	D=0	G=0 D=4	No spark-discharge.	
Methylamine, $\left.\begin{array}{c} CH_3 \\ H \\ H \end{array}\right\} N$	D=7	D=20 Gas-evolution.	D=5 Great heat and effervescence.	D=9 No spark-discharge.
Dimethylamine[5], $\left.\begin{array}{c} CH_3 \\ CH_3 \\ H \end{array}\right\} N$	D=6	....	Brisk decomposition and gas-evolution.	
Trimethylamine[6], $\left.\begin{array}{c} CH_3 \\ CH_3 \\ CH_3 \end{array}\right\} N$	D=0	D=4	Spark-discharge and much heat evolved.	
Amylamine[7], $\left.\begin{array}{c} C_5H_{11} \\ H \\ H \end{array}\right\} N$	....	D=10	Spark-discharge at 2 mm. distance of electrodes.	
Propylamine[8], $\left.\begin{array}{c} C_3H_7 \\ H \\ H \end{array}\right\} N$	G=0 D=0	....	Spark-discharge.	
Allylamine[9], $\left.\begin{array}{c} C_3H_5 \\ H \\ H \end{array}\right\} N$	D=2	....	Spark-discharge. D=10. Much heat.	

[1] Gas liberated by the galvanic current; liquid becomes blue.
[2] No spark-discharge.
[3] Spark-discharge when the electrodes were 3 millims. apart.
[4] With the electrodes 1 millim. apart, discharge of the induction-coil causes an undulating motion in the liquid.
[5] No spark-discharge with the electrodes 2·5 millims. apart.
[6] Electrodes 2 millims. apart.
[7] With a greater distance than 2 millims. agitation perceptible at the longer electrode.
[8] With the electrodes less than 2 millims apart, green-coloured sparks and agitation of the electrodes.
[9] Distance between electrodes=2 millims.

Table (*continued*).

Compound.	Galvanic battery.		Induction-coil.	
	20 B. el.	80 B. el.	Spark-length =15 millims.	Spark-length =70 millims.
Phenylamine[10], $\left.\begin{array}{l} C_6 H_5 \\ H \\ H \end{array}\right\} N$	$D=\overset{\circ}{0}$	Trace.	No action.	
Diphenylamine[11], $\left.\begin{array}{l} C_6 H_5 \\ C_6 H_5 \\ H \end{array}\right\} N$	$D=0$	......	Spark-discharge; electrodes 1 mm. apart.	
Ethylaniline[12], $\left.\begin{array}{l} C_2 H_5 \\ C_6 H_5 \\ H \end{array}\right\} N$	$D=0$	Trace.	Spark-discharge.	
Naphthylamine[13], $\left.\begin{array}{l} C_{10} H_7 \\ H \\ H \end{array}\right\} N$	$D=0$	$D=0$	$D=0°.$ No action.	No conduction.

[10] Distance between the electrodes=1 millim.
[11] Substance fusible; on the discharge, separation of carbon.
[12] Bright-green sparks pass; distance of electrodes = 2 millims.; separation of carbon.
[13] Fusible; electrodes 3 millims. apart; no discharge.

To this series belong two condensed gases only—the ammonia and the methylamine; the other substances are either liquid at ordinary temperatures or fusible without decomposition. Definite electrolysis takes place with ammonia, ethylamine, and methylamine, as even the discharge of 5 Bunsen elements is followed by a polarization-current. The compounds in which the alcohol-radical has replaced more than one atom of H (di- and triethylamine &c.) exhibit a diminution of conducting-power, which may be in accordance with the weakening of the capability of chemical reaction. Thus, in the secondary and tertiary amines the basic properties are less, the solubility diminished, &c. The phenylamine (aniline) series comprises very bad conductors; and, indeed, induction-sparks of 70 millims. will not strike through naphthylamine.

The chemical behaviour of these bodies in respect of easy exchange of hydrogen for metals is not always in accordance with their conductivity. In phenylamine, for example, hydrogen is directly expelled by potassium, copper and mercury compounds form at the ordinary temperature by elective affinity; while with gaseous ammonia the exchange of hydrogen for potassium and sodium, and also for silver, takes place only on the temperature being raised; and in the liquid state its

chemical properties seem to be lessened. It appears from Gore's observations [*] that this liquid possesses a tolerable solvent power for certain inorganic salts, as well as for potassium and sodium, but without being decomposed by them; Weyl found no alteration after it had been for twenty-four hours in contact with mercury and zinc; and yet ammonia exhibits a better conduction than phenylamine. The phenomena of elective affinity are of little importance here, since these bodies mostly act as bases without being themselves decomposed. For the double decompositions very high temperatures are often necessary,—for example, in the action of ammonia upon acetate of ethyl, forming acetamide at 130°—or upon chloracetyl, forming acetamide and hydrochloric acid, at not less than 120°. On the other hand, the action of phenylamine upon chloracetyl is very energetic at ordinary temperatures, with formation of acetanilide.

TABLE IV.—Electrolysis of the Amides.

Compound.	Battery 20 B. el.	Induction-coil. Spark-length = 15 mm.	Remarks.
Benzamide, $\left.{C_7H_5O \atop H_2}\right\}N$	$D = \overset{\circ}{90}$	$D = \overset{\circ}{30}$	Fusible; no spark-discharge, but rapid decomposition.
Acetamide, $\left.{C_2H_3O \atop H_2}\right\}N$	$D = 90$	......	Fusible.
Formamide, $\left.{CHO \atop H_2}\right\}N$	$D = 90$	$D = 15$	Liquid; brisk effervescence at the electrodes.
Butylamide, $\left.{C_4H_7O \atop H_2}\right\}N$	$D = 70$	$D = 80$	Fusible; electrodes 4 mm. apart; much gas evolved at both electrodes.

These bodies are excellent conductors; hence only feeble electromotive forces were employed for testing their electrolysis. In all of them there was very decided decomposition with evolution of gas. Moreover their chemical properties favour the above-mentioned hypotheses, as they readily exchange an atom of hydrogen for a metal; their decomposition by elective affinity, however, is mostly effected only with the aid of heat.

6. *Electrolysis of Organo-metallic Compounds.*

These are among the worst conductors of electricity; even a high tension is not always sufficient to overcome their resistance. Yet a current seems to pass through mercury-

[*] Proceedings of the Royal Society, vol. xx. p. 441, 1872.

methyl: I endeavoured to cause an induction-current to pass, of which the length of the spark in air was 15 millims., with the electrodes 4 millims. apart; no discharge through the liquid took place, even when the distance was reduced to 2 millims.; but the liquid was put into undulatory motion, while the longer electrode was set vibrating (see further on). When the electrodes were brought to 1 millim. distance from each other, the spark-discharge ensued.

TABLE V.

Compound.	Galvanic battery, 80 B. el.	Induction-coil. Spark-length = 17 mm.	Remarks.
Zincethyl, $(C_2H_5)_2$ Zn	$D=\overset{\circ}{0}$	$D=\overset{\circ}{0}$	No spark-discharge.
Mercuryethyl, $(C_2H_5)_2$ Hg	$D=0$	$D=0$	Undulatory motion of the liquid.
Zincmethyl, $(CH_3)_2$ Zn	$D=0$	Trace.	Slight rise of temperature.
Mercurymethyl, $(CH_3)_2$ Hg	$D=0$	Trace.	Slight rise of temperature, the electrodes 3 mm. apart, and spark-discharge.
Aluminiummethyl, $(CH_3)_3$ Al	$D=0$	$D=0$	Distance of electrodes 3 mm., agitation of the longer electrode.
Zincamyl, $(C_5H_{11})_2$ Zn	$D=0$	$D=0$	Dist. electr., 3 mm., strong agitation of longer electrode; spark-discharge at 2 mm. distance.

The insulating properties of these bodies cannot be reconciled with the above-mentioned hypotheses, according to which good conduction might be expected [*]. They are characterized by a very loose union of their constituents; they are readily decomposed by most reagents through elective affinity, or exchange their metals with one another even at ordinary temperatures. Thus from mercurymethyl with aluminium aluminiummethyl is formed; also the mercury is separated by zinc through double decomposition; with chloride of lead or mercury the corresponding ethyl-compounds are formed

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[*] Hittorf, in his treatise (*l. c.* p. 500), says on this subject:—"Frankland and Löwig succeeded in coupling even the most decidedly basic metals with alcohol-radicals. The compounds, which behave like the ordinary salts, will also be electrolytes; so far as I know, however, nothing has been published respecting their electrolysis." The above results do not confirm this opinion.

from zincethyl; water and dilute acids convert zincethyl into a hydrate and methyl, &c.

Some inorganic compounds of metals are known as very bad conductors, *e. g.* stannic chloride ($Sn Cl_4$); I obtained no deflection with a battery of 80 Bunsen elements. This may be in accordance with its chemical character, since stannic chloride is, in various reactions, almost always taken up as a group of atoms without being decomposed. The same remark holds good for the pentachloride of antimony and the chloride of arsenic, with which I have found Buff's results * confirmed with much more powerful currents. The combinations of the halogens with mercury are fusible, and yet are very bad conductors of electricity; nevertheless they are decomposed by most of the metals, and appear to be held together by only feeble affinity. Hittorf sought to bring these facts into harmony with each other, by remarking that the decompositions by double elective affinity are absent here, since the oxygen acids, which are regarded as the most powerful, will not decompose these compounds either diluted or concentrated, either cold or with the aid of heat. But this remark is not applicable to the organo-metallic compounds, upon which acids, whether concentrated or diluted, immediately act, and furnish new bodies as the products of decomposition. Thus, for example, zincethyl gives with hydrochloric acid chloride of zinc and methyl; mercuryethyl with diluted sulphuric acid gives methyl and a compound of sulphuric acid and ethyl, &c. Consequently it follows from this series of experiments that no simple relation can here be demonstrated between chemical decomposition and electrolysis.

[To be continued.]

LIV. *Notices respecting New Books.*

Die Potentialfunction und das Potential, ein Beitrag zur mathematischen Physik von R. CLAUSIUS. Dritte vermehrte Auflage. Leipzig, 1877. Verlag von Johann Ambrosius Barth. (Royal 8vo, pp. 178.)

PROFESSOR CLAUSIUS states that the object of his treatise is to make the reader acquainted in the simplest possible manner with the function to which Greer gave the name of the Potential function. He does this by developing the subject very systematically from first principles, dwelling with great exactness and perfect clearness on every point that is likely to give the reader

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* Pogg. *Ann.* vol. cx. p. 267, 1859. Buff found no conduction in these bodies with a battery of 16 elements, and holds that stannic chloride is an absolute nonconductor. Lapschin and Tichanowitsch likewise observed no conduction with the current of 370 elements in oxychloride of antimony.

trouble, or which might be imperfectly apprehended unless attention were distinctly drawn to it.

Beginning with the case in which a movable point is acted on by any forces, he first limits the question to the case in which the resultant force can be expressed by means of a force-function, then by a further limitation to the case in which the forces concerned are central, and finally to the case in which the force-function becomes the potential function. From this point the subject is treated with great minuteness in regard to all that relates to the determination of the function itself and its first and second differential coefficients. The case in which the point is within the attracting body comes in for very detailed notice, such as its importance deserves and requires; for then, as is well known, the function under the integral sign becomes infinite within the limits of integration. Accordingly, for example, the fundamental equation

$$\Delta^2 V = -4\pi\epsilon k \quad . \quad . \quad . \quad . \quad . \quad . \quad (1)$$

is first proved, in what we may call the usual manner, both for a homogeneous body and for a body not homogeneous. Not content with this, however, the author reproduces a proof, which he first published in Liouville's Journal in the year 1858, and which, as he says, " seems to me to avoid in the simplest manner the difficulty arising from the function under the integral sign becoming infinite " (p. 37). The method adopted, it may be added, depends in the first place on effecting a certain transformation of the expressions for the force-components, and then on proving the proposition in question by means of these expressions, first for a homogeneous body and then for a body not homogeneous. At the end of the proof the author adds the remark, which expresses the spirit of the whole book, " We have thus arrived at the equation to be proved by means of a series of perfectly simple mathematical operations which depend only on the fundamental principles of the differential and integral calculus " (p. 48).

The author makes great use of certain terms which he has introduced, and which call for notice. In the simplest view of the subject the potential function is merely a function from which can be easily derived the attraction exerted by a number of particles— whether forming a continuous body or not—on a particle in any assigned position in space. Under this point of view, to speak of the potential at any point of space of a given distribution of matter is not open to objection; but when the properties of the potential function are applied to questions of magnetism or electricity, there is a certain impropriety in speaking of a distribution of electricity as a distribution of matter. Professor Clausius proposes to avoid this impropriety by using the terms *Agens* and *Menge*. The word *Agens* is used as the genus of which ponderable matter, electricity, magnetism, &c., are the species. The word *Menge* is used instead of the word mass or quantity of matter, in order to avoid introducing the notion of inertia, which is inextricably bound up with

that of matter. "No other supposition is made in regard to an Agent (*Agens*) except that it can be determined as to quantity, and that the force exerted by a certain group of an Agent (*Menge eines Agens*) is proportional to the group (*Menge*), other circumstances being the same" (p. 9). This point is one of considerable importance, as these words, one or both of them, appear in nearly all the leading enunciations of the book ; *e. g.*, it is said of equation (1) that " it expresses the second leading property of the potential function, viz. that from the potential function of an agent we can deduce its density as a function of the space-coordinates, and thereby can determine the way in which the agent is distributed through space" (p. 34).

It is plain from the titlepage that Professor Clausius draws a distinction between the potential function and the potential : in fact, the work consists of two parts of unequal length, the first and longer being devoted to the former subject, the second to the latter. The potential function is defined thus :—" When an agent acts by attraction or repulsion, according to the inverse square of the distance, its force-function relative to a unit of the same agent, supposed to be concentrated in a point, is called the Potential function " (p. 12); and accordingly it is expressed by

$$V = \epsilon \Sigma \frac{q'}{r} \text{ or by } V = \epsilon \int \frac{dq'}{r}.$$

The constant ϵ, which appears throughout the work, is defined to be the force of repulsion which two positive units of the *agent* exert on each other at the unit of distance ; and if we choose as the unit of the *agent* that group (*Menge*) which exerts the unit of force on an equal group (*Menge*) of the *agent* at the unit of distance, we shall have $\epsilon = -1$* for ponderable matter, and $\epsilon = +1$ for electricity and magnetism (p. 15).

The potential (W), which forms the subject of the second part of the treatise, is the force-function of one group (*Menge*) relative to another *group*, not merely relative to a unit *group* conceived as concentrated at a point ; it is, accordingly, represented by a double summation, viz.

$$W = \epsilon \Sigma \frac{qq'}{r} \text{ or } W = \epsilon \iint \frac{dq\, dq'}{r},$$

where q belongs to the one *group* and q' to the other. It is plain that

$$W = \int V' dq = \int V dq';$$

in other words the potential is obtained from the potential function

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* Of course, therefore, in the case of ponderable matter, equation (I.) takes the form $\nabla^2 V = 4\pi k$; this equation is usually written $\nabla^2 V = -4\pi k$; but then Professor Clausius expresses the components of the force along the coordinate axes by $-\dfrac{dV}{dx}$ &c.

by integration ; its importance arises from the fact that it serves to express the work done by the forces which are found to be actually exerted in Nature. In the second part of the work Professor Clausius states with great clearness the various steps by which the Potential is at length arrived at. Thus, beginning with a very careful statement of the principle of Virtual Velocities, he passes on through D'Alembert's principle to the proposition of the Equivalence of Vis Viva and work, and thus arrives at the notion of the " ergal," *i. e.* the function (Ω), if such a function exists, given by the equation

$$\Sigma(\mathrm{X}dx + \mathrm{Y}dy + \mathrm{Z}dz) = -d\Omega.$$

He then discusses two cases in which the forces of a system in motion have an " ergal," and finally arrives at the potential by a limitation exactly resembling that by which the potential function is obtained from the force-function.

LV. *Proceedings of Learned Societies.*

GEOLOGICAL SOCIETY.

[Continued from p. 316.]

February 20, 1878.—Henry Clifton Sorby, Esq,, F.R.S., President, in the Chair.

THE following communications were read :—

1. " Notes on the Physical Geology of the Upper Punjáb." By A. B. Wynne, Esq., F.G.S.

The author stated that crystalline rocks are rare in the accession parts of the Upper Punjáb district, and that when present they consist of syenite and gneiss. The Cambrian and Silurian formations are represented by more or less metamorphosed azoic slates in the Himalayan district, and in the Salt range by a zone less than 200 feet thick, containing either *Obolus* or *Siphonotreta*, underlain by a thick unfossiliferous sandstone, beneath which is a deposit of gypseous marl and salt. Above the Silurian in the Salt range, and conformable to it, comes the Magnesian Sandstone group and a group of unfossiliferous sandstones and clays ; in the Himalaya these deposits are probably represented by an unfossiliferous siliceous dolomite, which rests unconformably upon the slates. There are no fossils indicative of rocks of Devonian age. The Carboniferous rocks are also conformably deposited on limestones, sandstones, and shales, the last sometimes carbonaceous. These deposits contain hæmatite in pockets ; and the oldest known Ammonites have been found in them. An infra-Triassic group occurring in Lei Bau mountain consists of red shales, sandstones, and red quartzitic dolomites, overlain by lighter-coloured siliceous dolomites, which in their turn are covered by hæmatite, quartz breccia, sandstones, and shales. The author believes these to have been deposited by the same waters which subsequently laid down the Trias, which is largely composed of limestones in the northern Himalayan area, and here and elsewhere includes dolomites, shales, and sandstones. Numerous fos-

sils occur in some of the beds, such as *Dicerocardium*, *Megalodon*, and *Nerinæa*.   In the western part of the Salt range conglomerates composed of great blocks are regarded by the author as evidence of proximity of land.   The Jurassic deposits are local in their distribution, and consist of shales, sandstones, and limestones, containing abundant fossils, such as Belemnites, Ammonites, and Saurians.   A dark limestone contains also *Gryphœæ* and *Trigoniæ*.   The Cretaceous deposits, when present, are conformable to the Carboniferous; they are variable in thickness and fossil contents, and are not recognizable near Attock between the Jurassic and Nummulitic groups.   Further east a group, supposed to be Cretaceous, includes clays with boulders of crystalline rock, which the author regards as derived from land to the south.   One of these boulders presented glacial striæ.   The Eocene rocks are generally limestones, and lie conformably upon the subjacent formations.   The Nummulitic series of the Salt range includes gypseous and coaly shales.   The salt beds sometimes attain a thickness of over 1000 feet.   The Miocene and Pliocene deposits are of immense thickness, and contain only fossils of terrestrial and freshwater origin, so that the deposits were formed in lakes and inland seas.   The Tertiary epoch closed with the elevation of the Himalayas and Salt range, which was followed by a long period of change, during which various deposits were produced, some including great quantities of erratics, which, however, the author believes were brought to their present position rather by floating ice than by the extension of glaciers.

2. " Description and Correlation of the Bournemouth Beds.— Part I. Upper or Marine Series." By J. Starkie Gardner, Esq., F.G.S.

The author stated that nothing had been written on this subject since Prof. Prestwich's paper, in which the beds at Hengistbury were described as of the Barton series.   No attempt had hitherto been made to correlate the beds at Alum Bay or Whitecliff Bay with those of the mainland ; and no reference was to be found anywhere to the origin and sequence of the beds between Hengistbury Head and Bournemouth, or to their contained fossils.   He had now correlated these bed for bed with the strata at Alum Bay, and found that there is a sequence, and that the Hengistbury beds are higher than those of Bournemouth and do not reappear on the coast. They are all of marine origin, and were deposited by a sea advancing from the south, as is shown by the slope of the shingle beds and the lenticular patches of clay which mark old channels parallel to the former shore and at right angles to the present cliff-line.   They contain numerous fossils—fruits, leaves, Mollusca, and Crustacea—the fruits resembling those of Sheppey and forming a group of similar character.   The Mollusca are of Bracklesham type ; and the fossils include three genera of Bryozoa, two of which are new to the Eocene. The Crustacea have not yet been examined.

The author comes to the conclusion that the whole group is contemporaneous with the Bracklesham beds, and is not of Lower-

Bagshot age. Similar shore-conditions probably extended into the London basin; and the beds mapped by the Survey as Lower Bagshot are probably of the same age as those at Boscombe, in which case nothing more than the Bracklesham is to be met with in the London basin. The similarity of the leaves, &c., from Bovey Tracy to those obtained by the author leads him to infer that the former also are of Eocene, and not of Miocene age. The author increases the thickness of the London Clay at Alum Bay at the expense of the Bagshot beds, and diminishes that of the Bracklesham beds at Whitecliff Bay by transferring part of them to the Lower Bagshot.

3. "Notes on certain Modes of Occurrence of Gold in Australia." By Richard Daintree, Esq., F.G.S.

The author stated that he had in a previous paper (Q. J. G. S. vol. xxviii. p. 271) proved the occurrence of gold in the Devonian rocks of Queensland, and, further, that the auriferous tracts were certainly confined to those districts in which the Devonian rocks were penetrated by certain plutonic rocks, principally pyritous diorites. These conclusions had since been confirmed by Mr. W. C. Wilkinson and Dr. G. H. F. Ulrich; and the facts thus established are of the greatest practical importance to miners. With regard to the epoch when the auriferous pyrites was deposited in the rocks, the author expresses the opinion that most of the pyrites is contemporaneous with the consolidation of the rocks in which it occurs, although some may have been subsequently introduced by infiltration; but this is not common in Australia. A more common case is the separation of gold generally diffused through a rock into local fissures, forming strings and veins. The author thinks that all the evidence goes to show that the Australian auriferous veins were chiefly formed during the earliest era of great volcanic agitation indicated by the condition of the stratified rocks, namely the Devonian, but that they were enriched during a subsequent Tertiary (probably Miocene) period of intense activity. No traces of auriferous veins have yet been found in any Mesozoic or Cainozoic deposits in Australia.

4. "Notes on the Geology of the Island of Mauritius and the adjacent Islets." By W. H. T. Power, Esq., B.A.

The author stated that the island of Mauritius consists of an elevated central plateau, bounded by an incomplete wall of volcanic rock, round part of which there is a coral reef and coral sand-rock, and also rocks of various colours produced by the decomposition of volcanic rocks. Outside is a living coral reef. In the middle of the island the old crater-wall can be distinguished, although broken; two secondary craters are also noticed. On the north slope of the island there is a flow of columnar basalt to the sea. There is an opening in the old coral-reef, as in the existing one opposite the mouth of the Black River. Gabriel Island consists of a coral reef and detrital coral rock upon a foundation of basalt, the section showing in descending order :—1. Coral stone; 2. Conglomerate of

coral, with some basalt pebbles and shells ; 3. Compact limestone, with thin layers of basalt at base. The author described the supposed fossil trees noticed in this island by Messrs. Ayres and Clarke (Q. J. G. S. xxiii. p. 185) as composed simply of hard portions of coral rock left outstanding by the weathering of the softer intervening parts ; they show the same stratification as the rock below. The islet known as Gunner's Quoin consists of co-lumnar basaltic lava, capping volcanic sand, below which is a browner volcanic sand with seams of coral fragments. Flat Island is in part the remains of a volcanic crater, and the rest consists of volcanic sand strewn with coral blocks. There are basaltic dykes in the hill, the top of which appears to show traces of one or more plugs. The author concludes that Mauritius was once an active volcano, now elevated with the old reef. The islets also formed part of a volcano or volcanoes, and have also been elevated with reef-material.

LVI. *Intelligence and Miscellaneous Articles.*

ON THE PITCH OF A TUNING-FORK IN AN INCOMPRESSIBLE FLUID. BY FELIX AUERBACH.

WHEN a tuning-fork is struck and then quickly immersed in a vessel containing water, there is heard, especially if the ear be applied to the resonant table, a tone the height of which does not accord with that of the tuning-fork in air.

This phenomenon, which seems to have hitherto excited but little notice (I have found a remark in reference to it only in Chladni's *Akustik*), follows as a necessary consequence from the fact that the dissipation of the kinetic energy takes place in the so-called incompressible fluids under other circumstances than those which subsist in gases. For into all formulæ for the velocity of propagation and for the number of vibrations enters the square-root of a quantity to which a determinate signification cannot at once be assigned. In fact the coefficient of elasticity is usually defined merely as the ratio of an infinitesimal increment of pressure to the corresponding infinitesimal compression of volume ; yet its value essentially de-pends on what are the circumstances under which the change of state in question proceeds. Of special importance are two cases— namely, that in which the entropy of the system, and that in which the temperature remains constant. With the sonorous vibrations of elastic solid bodies in gases the first of these cases is approxi-mately realized. For the alternate condensations and rarefactions undergone by the gas have, it is true, for their consequence changes of the temperature ; hence, in the equalization of this, kinetic energy is given up to the environment ; but the conductivity of gases for heat is so inconsiderable that the dissipation of energy can be neglected for rapid vibrations like those of sound : for these, according to Clausius, it may even be said that the entropy remains

constant. The other case is to a certain degree of approximation realized when the solid body vibrates in a liquid the surface of which is in part free (that is, bounded by an elastic fluid). As in the other case the variations of the entropy were negligible, so in this are the changes of temperature, and, indeed, for three reasons. In the first place, the condensations are very slight; therefore no molecular energy is generated. Secondly, the specific heat of water is very great; consequently the temperature is disproportionately little altered. And, thirdly, the conductivity of water for heat is not so inconsiderable that a considerable part of the changes of temperature which might by chance take place would not be again eliminated by equalization.

The two values of the coefficient of elasticity in these two cases are in the same proportion to each other as the two values possessed by the specific heat, according as the pressure or the volume remains constant. This ratio does not materially differ from 1·4.

From these considerations the nature and quantity of the change of tone can be deduced which took place in the experiment mentioned at the commencement. The tone must become deeper; for with the temperature constant the elasticity is less than with a constant entropy; and the tones in air and in water must be proportional to one another as $\sqrt{1·4} : 1$, *i. e.* as 1·18 : 1, or about as 7 : 6. The interval must therefore be greater than an entire tone, and less than a minor third.

Indeed experiments show that the interval approximates to this value required by theory as a maximum. That on the average it is somewhat less (viz. for the middle notes exactly a complete tone) is not surprising when it is remembered that the above considerations hold for two ideal extreme cases, *between* which lie those of vibrations in air and in water. For a few tones I have determined the deepening by making use of the beats which are heard when simultaneously with the fork immersed in water another fork, the tone of which is a complete interval deeper than the tone in air of the experiment-fork, is set vibrating in air. In this way I found for the ratio of vibration with the following tones the values placed under them.

c (132 vibr.).	c_1 (264 vibr.).	g_1 (396 vibr.).	c_2 (528 vibr.).
1·11 (<9 : 8)	1·12 (9 : 8)	1·13 (<8 : 7)	1·15 (>8 : 7)

The result derived from these numbers, namely that the ideal extreme value of the interval is the more closely approximated to the higher the tone (*i. e.* the shorter the period of vibration), agrees, according to the foregoing, with the theory.

In conclusion, I have instituted some control-experiments with other liquids. From these it follows that the influence of the resistance of the liquids, which depends on their density and viscosity, and which is very important for the amplitude and the logarithmic

decrement of the tuning-fork's vibrations, does not sensibly come into consideration with respect to the length of a vibration—not even in the cases of mercury and glycerine, although in the former the density, and in the latter the viscosity is very great.—*Annalen der Physik und Chemie*, 1878, No. 1, pp. 157–160.
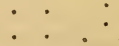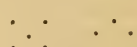
A NOTE ON EXPERIMENTS WITH FLOATING MAGNETS ; SHOWING THE MOTIONS AND ARRANGEMENTS IN A PLANE OF FREELY MOVING BODIES ACTED ON BY FORCES OF ATTRACTION AND REPULSION, AND SERVING IN THE STUDY OF THE DIRECTIONS AND MOTIONS OF THE LINES OF MAGNETIC FORCE. BY ALFRED M. MAYER.

For one of my little books of the Experimental-Science Series I have devised a system of experiments which illustrate the action of atomic forces, and the atomic arrangement in molecules, in so pleasing a manner, that I think these experiments should be known to those interested in the study and teaching of physics.

A dozen or more of No. 5 or 6 sewing-needles are magnetized with their points of the same polarity, say north. Each needle is run into a small cork, $\frac{1}{4}$ in. long and $\frac{3}{16}$ in. in diameter, which is of such size that it just floats the needle in an upright position. The eye end of the needle just comes through the top of the cork.

Float three of these vertical magnetic needles in a bowl of water, and then slowly bring down over them the north pole of a rather large cylindrical magnet. The mutually repellent needles at once approach each other, and finally arrange themselves at the vertices of an equilateral triangle, thus ∴.. The needles come nearer together or go further away as the magnet, above them, approaches them or is removed from them. Vibrations of the magnet up and down cause the needles to vibrate ; the triangle formed by them alternately increasing and diminishing in size.

On lifting the magnet vertically to a distance, the needles mutually repel and end by taking up positions at the vertices of a triangle inscribed to the bowl.

Four floating needles take these two forms

Five   ,,   ,,   ,,   ,,   ,,   ,,

Six   ,,   ,,   ,,   ,,   ,,   ,,

Seven   ,,   ,,   ,,   the form

I have obtained the figures up to the combination of twenty floating needles. Some of these forms are stable ; others are unstable, and are sent into the stable forms by vibration.

These experiments can be varied without end. It is certainly interesting to see the mutual effect of two or more vibrating systems, each ruled more or less by the motions of its own superposed magnet, to witness the deformations and decompositions of one molecular arrangement by the vibrations of a neighbouring group, to note the changes in form which take place when a larger magnet enters the combination, and to see the deformation of groups produced by the side action of a magnet placed near the bowl.

In the vertical lantern these exhibitions are suggestive of much thought to the student. Of course they are merely suggestions and illustrations of molecular actions and forms ; for they exhibit only the results of actions in a plane ; so the student should be careful how he draws conclusions from them as to the grouping and mutual actions of molecules in space.

I will here add that I use needles floating vertically and horizontally in water as delicate and mobile indicators of magnetic actions—such as the determination of the position of the poles in magnets, and the displacement of the lines of magnetic force during inductive action on plates of metal, at rest and in motion.

The vibratory motions in the lines of force in the Bell telephone have been studied from the motions of a needle (floating vertically under the pole of the magnet) caused by moving to and fro through determined distances the thin iron plate in front of this magnet. These experiments are worth repeating by those who desire clearer conceptions of the manner of action of that remarkable instrument.—Silliman's *American Journal*, April 1878.

ON SENSATIONS OF LIGHT AND OF COLOUR, IN DIRECT AND INDIRECT VISION. BY E. LANDOLT AND A. CHARPENTIER.

It is known that the fibres of the optic nerve expand at the back part of the eye, forming by their terminations a nervous membrane of hemispherical form which is termed the *retina*. It is upon this that the images of external objects are depicted as in a camera obscura ; and, undergoing certain modifications under the influence of those images, it is the starting-point of the luminous sensations. Now the eye utilizes for direct vision scarcely any of these images but those formed upon the central part of the retina, named the *fovea centralis*, and corresponding to the fixation-point ; the rest are perceived more or less vaguely. There is therefore reason for distinguishing between direct and indirect vision—the first, much more distinct, corresponding to the objects at which we look, and the second to the other objects in the visual field.

Does this very clear difference between direct and indirect vision depend on a real difference of sensibility in the different parts of the retina ? This question we proposed to ourselves to determine, having been prepared for the determination by the numerous series of investigations already made by one of us upon the subject [*].

* See Landolt, *Ophthalmométrologie*, 1874.

We employed a method the principle of which had been indicated by the latter, and which is that used in experimental physiology to test the excitability of nerves. This method consists in determining the minimum of excitation necessary to be applied to the nerves in order that they may react. Now light is the normal excitant of the optic nerve, and the luminous sensation its special mode of reaction. The question therefore was, to determine, for the centre of the eye and for points more and more excentric, what is the minimum of light that must be presented to the eye to obtain a luminous sensation. But the eye perceives not only light, but also colours; so it was necessary to repeat for the monochromatic rays the same experiments as for white light.

To accomplish these two different ends we made use of a very simple instrument, contrived by one of us*, consisting essentially of a convex lens producing on a roughed plate of glass the image of a luminous object. By utilizing, with the aid of a special diaphragm, various extents of the lens, images are obtained, always distinct, but differently illuminated; and their relative illumination can be estimated with quite sufficient exactness from the extent of lens employed to produce them. The luminous object consists of another roughed glass plate of determined superficies, uniformly illuminated with the kind of light, white or coloured, the effect of which we wish to try.

The experiment consists in viewing in the dark the image formed by the object upon our first screen, while the eye fixes successively the image itself and points more and more distant from it. For each position of viewing, the observer determines what is the minimum of luminous intensity necessary for our image to produce a sensation of light or colour.

Such is essentially the method we have employed for determining the excitability of the different parts of the retina, and which has given the following results :—

Touching the purely luminous sensibility of the different points of the retina, we have constantly found necessary, for the centre and for each of those points, the same minimum of white light in order to produce a luminous sensation. The luminous sensibility is therefore the same for every part of the retina.

It is otherwise if, instead of exciting the retina with white light, we excite it with monochromatic light. Then it is seen that, in order to distinguish the colour presented to the eye, we do not require that colour to possess so much intensity for the centre as for the rest of the retina, and that, the further we remove from the point of fixation (that is, from the centre), the more intense must the colour be, in order to be recognized.

But, a remarkable thing, before any colour is recognized with its true tone it appears always to pass through a series of phases, the first of which is interpreted by a *purely luminous sensation*; then one hesitates about the quality of the colour presented, until the excitation has attained a certain intensity, with which the colour

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* See Charpentier, *Société de Biologie*, Feb. 17, 1877.

is recognized. Now we have found, in all our experiments, this important fact, that the production of the initial luminous sensation requires, for the centre and all points of the rest of the retina, the same minimum of the colour presented.

This would seem to prove that the chromatic is a function distinct from the luminous sensation, both in its seat and in its nature. Indeed we have seen that any luminous excitation whatever always commences with producing a simple luminous sensation, while we must always have an intenser excitation in order to get a sensation of colour,—that the minimum of excitation necessary to produce the luminous sensation is constant for the whole extent of the retina, while the minimum of excitation necessary to produce chromatic sensations is by so much greater the more excentric the part of the retina interrogated.

These facts would be explained if it were admitted, as one of us has sought to show in a previous work[*], that the colour-sensations are, in great part, the result of a special elaboration, made by the nervous centres, of the impressions transmitted to them by the retina—an elaboration which only comes after the simple primitive sensation produced by any luminous excitation. For the parts of the retina which we exercise the most, like that which corresponds to the fixation-point, the intermediate phase which exists between the simple luminous sensation and the consecutive chromatic elaboration is almost nothing, although it can be made evident by certain methods; while the less the retinal part put in action has been exercised, which is the case for the excentric portions upon which we do not ordinarily fix our attention, the slower and more difficult to produce is the chromatic elaboration.

As to the enormous imperfection of indirect vision, we have previously proved that it affects solely the faculty of distinguishing forms, which appears to be due to the following anatomical peculiarity: the centre of the retina receives, within an equal space, a much greater quantity of nervous fibres than the excentric parts, and consequently can convey to the brain a much greater number of distinct impressions.—*Comptes Rendus de l'Académie des Sciences*, Feb. 18, 1878, tome lxxxvi. pp. 495–497.

ON THE GALVANIC POLARIZATION OF PLATINUM IN WATER.
BY DR. FRANZ EXNER.

The main conclusion drawn from the investigation refers to the amount of the polarization called forth by currents whose electromotive force is too insignificant to produce persistent decomposition of the water. For this hitherto virtually uninvestigated case the law is deduced that the electromotive force of the polarization is always exactly equal to that of the primary current. The paper contains besides explanations respecting the causes which have occasioned such varying values to be given by different authors for the so-called maximum of polarization.—*Kaiserliche Akademie der Wissenschaften in Wien, math.-naturw. Classe*, Feb. 28, 1878.

[*] Charpentier, *De la Vision avec les différentes parties de la rétine*, 1877.

LVII. *Some Electrical Experiments with Crystalline Selenium.*
By ROBERT SABINE[*].

THE following experiments were undertaken with the view of removing, if possible, some of the difficulties which I had found in the way of constructing constant resistances of crystalline selenium.

For convenience, I propose to retain the order in which the experiments suggested themselves and were made, and shall therefore divide the following account of them into (1) the experiments with crystalline selenium in darkness, and (2) its behaviour in light.

The experiments were made with several specimens of selenium, some of which were provided, when in the amorphous state, with platinum wires. The process of annealing was done in an iron pot contained in an oil-bath, the temperature of which could be kept steady at any required degree. The selenium was made in the form of plates about 0·1 centim. thick, 0·5 to 1 centim. broad, and 2 to 3 centims. long, wires (when employed) being inserted transversely at equal distances apart. In some specimens, the wires were laid upon a small piece of mica or platinum-foil, and the melted selenium dropped upon them; in others the platinum wires were heated to incandescence in a smokeless flame, and, while still hot, were imbedded in the amorphous selenium. The two methods appeared to be capable of giving equally firm contact, which was probably due chiefly to the contraction of the selenium round the platinum wire.

In preparing the selenium plates, care was taken to obtain as good a surface as possible. It was found that this might be effected by melting the amorphous selenium between edge strips of glass upon a piece of bright platinum foil, upon which it was annealed ; the unequal contraction of the selenium and platinum prevented any adhesion after removal from the annealing-pot, whilst the selenium acquired a bright metallic surface.

Mercury Contacts.—It is necessary to discriminate between the resistance of the selenium and the resistance of the junctions. A plate of selenium was annealed at 200° C. with platinum end wires. Each end was then inserted in the side of a cup formed by a pill-box, where it was secured by means of shellac in such a way that mercury could be introduced into the cup to make connexion with the measuring-apparatus. Before the mercury was poured in, the resistance between the platinum wires was 124,300 ohms. After the cup at one end was filled with mercury, the resistance was found to be reduced to 82,300 ohms ; and when the second cup was supplied with mercury, the resistance further diminished to 60,100 ohms.

In a subsequent experiment, a plate of selenium with platinum-wire connexions, having a resistance of 20 megohms, was found to have only 14 megohms resistance when the buttends were in contact with mercury. The resistances of some plates, however, were not greatly altered by this application of mercury connexions at the ends.

Resistance of Junctions, and want of homogeneity of the material.—The resistance of the junctions between the selenium and the platinum wires imbedded in it was found to be very various, but not to depend upon the manner in which the wires were imbedded in the selenium previous to its being crystallized. It was also found that the conducting-power of the selenium differed materially in different parts of the same plate.

Fig. 1 represents a plate of selenium with three platinum wires (1, 2, 3) imbedded transversely in it. The resistance of one of the wires was found as follows :—If x is the resistance of junction 2 under examination, r and r' the resistances of the neighbouring junctions plus whatever selenium may be between them and the wire 2, the measured resistances are :—

Fig. 1.
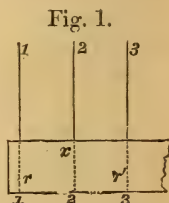
$$\text{Between 1 and 2,} \quad \text{R} = r + x \\ \text{,,} \quad 1 \text{ ,, } 3, \quad \text{R}_1 = r + r' \\ \text{,,} \quad 2 \text{ ,, } 3, \quad \text{R}_2 = x + r' \Bigg\} x = \frac{\text{R} - \text{R}_1 + \text{R}_2}{2}.$$

The resistance of each of the wires (except the end ones) with which the plates of selenium were provided was approximately ascertained in this way; and, by deducting the resistances of the junctions, the resistance of the intervening selenium was also ascertained.

A plate of selenium was provided with four platinum wires 0·5 centim. apart, annealed at 170° C. The resistances were measured with + and − currents, and the means assumed to be sufficiently near for the purpose.

Between wires.	Measured resistances.
1 and 2	31 megohms.
2 „ 3	162 „
3 „ 4	11 „
1 „ 3	174 „
2 „ 4	172 „

From these measurements the separate calculated resistances are :—

Junction number.	Resistance of junction.	Resistance of selenium between junctions.
2	9·5 megohms }	152 megohms.
3	0·5 megohm }	

This is an instance of high selenium resistance in the middle and low resistance towards the ends in an otherwise apparently homogeneous plate, and of low junction resistances. The two junctions were made at the same time, in the same manner, by melting the selenium upon the wires; and yet one of them has nearly twenty times the resistance of the other.

A second plate of selenium, provided with six platinum wires and annealed at 150° C., was measured with + and − currents in the same way, mean values being taken. This gave the following results :—

Junction number.	Resistance of of junction.	Resistance of selenium between junctions.
2	429 megohms }	22 megohms.
3	479 „ }	13 „
4	498 „ }	0 „
5	428 „ }	

In this plate, therefore, nearly all the resistance was situated in the junctions, whilst the selenium offered a comparatively small resistance, its conducting-power being much greater at one end than at the other.

A third plate was provided with seven platinum wires 0·7 centim. apart, annealed at 205° C. It showed electrically more homogeneity of material and equality of junction resistances.

Junction number.	Resistance of junction.	Resistance of selenium between junctions.
2	·0333 meghom	0·1914 megohm.
3	·0553 ,,	0·1045 ,,
4	·0328 ,,	0·1233 ,,
5	·0199 ,,	0·1084 ,,
6	·0285 ,,	

It is clear from these measurements that a large portion of the observed resistance of a so-called selenium resistance may, and frequently does, reside in the junctions and not in the selenium. Therefore the larger we make the surface of contact between the platinum and the selenium, the less likely are we to find an otherwise sensitive piece of selenium rendered comparatively insensitive by the introduction of high junction resistance. In this respect the form of selenium plate designed by Dr. Werner Siemens, in which the platinum wires form gratings or interlying spirals, is unquestionably the best form to employ when the object in view is to obtain a high sensitiveness to light.

Steady variations of Temperature.—The following experiment had for its object to ascertain whether the alteration of resistance accompanying a steady [*] variation of temperature is confined to the selenium or to the junctions, or is participated by both. The following data were obtained with a plate furnished with four platinum wires (Nos. 1 to 4), annealed at 200° C., measured in a Wheatstone-bridge with + current of 2 Daniell cells, after it had been at rest in the dark for several hours at each temperature.

Temp.	Measured resistance between wires.				
	1 and 2.	1 and 3.	2 and 3.	2 and 4.	3 and 4.
° 7·5 C.	meg. ·1641	meg. ·2691	meg. ·1446	meg. ·2429	meg. ·1521
15·5	·2168	·3565	·1905	·3201	·2003
21·0	·2343	·3865	·2068	·3473	·2225
32·2	·2739	·4549	·2429	·4092	·2577

The several resistances of junctions and selenium are therefore as follows :—

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[*] Unsteady and sudden variations of temperature gave very anomalous results. One plate of selenium, the resistance of which *increased* with a steady increase of temperature, was found to *decrease* in resistance for a few seconds by a sudden application of heat.

Temp.	Resistance of		
	Junction **2**.	Junction **3**.	Selenium be-tween **2** and **3**.
$\overset{\circ}{7\cdot5}$ C.	megohm. ·0198	megohm. ·0269	megohm. ·0979
15·5	·0254	·0354	·1298
21·0	·0273	·0410	·1385
32·2	·0309	·0457	·1663

It appears from this, that the resistances of junctions and selenium are both affected by variations of temperature in nearly an equal ratio.

Resistance of Selenium altered by the inversion of the Current. —The fact that the current strength in the circuit of a bar of selenium and a battery is subject to change when the direction of the current is reversed has been pointed out by Professor Adams and Mr. Day. I made an attempt to determine whether this change is due to electromotive force or to resistance, by carefully measuring the change on inversion while the current in the selenium was kept constant; but the total resistance of the circuit and the battery were increased in equal ratios. By this means the change, whatever it is, remains constant while all the other factors are different; and when the change is sufficient, there should be no difficulty in calculating it both as resistance and as electromotive force and discriminating between the two suppositions. Unfortunately, however, the majority of specimens of crystalline selenium did not alter sufficiently to afford definite evidence; and those recently prepared specimens which showed a considerable change, generally gave unsteady readings.

Seat of the Change.—The inquiry naturally occurs whether the seat of the change is in the selenium or at the junctions. To determine this, it is only necessary to ascertain the resistance of the junctions and of the selenium separately with two different battery powers to find which agrees best in the two measurements.

Fig. 2.

A plate of selenium was carefully measured between its four wires (marked *a, b, c,* and *d*) inserted in a Wheatstone bridge with an intervening commutator, so that the selenium could be inverted whilst the other members of the bridge remained unaltered. The side containing the selenium (*s*), fig. 2, was also furnished with a reflecting galvanometer
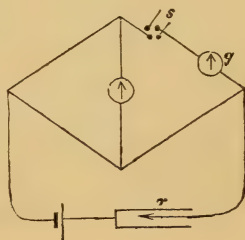
(g) of comparatively low resistance, by means of which the current moving in that side could, within a very small percentage of error, be observed. The battery-circuit was provided with a sliding resistance (r), by means of which, whichever section of the selenium plate was in circuit, the current in it, as indicated by galvanometer (g), could be kept at a constant value. In this way the following measurements were made:—In the first series the current-strength in the selenium was kept uniformly at 2·9 microwebers, and in the second at 0·42 microweber. The positions of the selenium are indicated as "direct" and "inverted"—" direct " when the current moved in the selenium as shown by the arrow (fig. 3), "inverted " when it moved the other way.

Fig. 3.
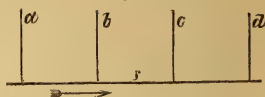
Resistance measured between	Current = 2·9 microw.		Current = 0·42 microw.	
	Direct. meg.	Inverted. meg.	Direct. meg.	Inverted. meg.
a and b	·3176	·3181	·3202	·3244
b ,, c	·3864	·3858	·3936	·3893
c ,, d	·2734	·2734	·2747	·2782
a ,, c	·6095	·6097	·6116	·6116
b ,, d	·4842	·4846	·4906	·4900

Call the resistance of the selenium between b and c $\left\{ \begin{matrix} s \\ s' \end{matrix} \right\}$ when the current is $\left\{ \begin{matrix} \text{direct} \\ \text{inverted} \end{matrix} \right\}$; the resistance of the junctions $\left\{ \begin{matrix} b \text{ and } c \\ b' \text{ and } c' \end{matrix} \right\}$ when the current is $\left\{ \begin{matrix} \text{direct} \\ \text{inverted} \end{matrix} \right\}$; the resistance from a up to the junction b $\left\{ \begin{matrix} a \ \ldots\ldots \text{ direct} \\ a' \ \ldots\ldots \text{ inverted} \end{matrix} \right\}$; and, lastly, the resistance from the junction c to the end d $\left\{ \begin{matrix} d \ \ldots\ldots \text{ direct} \\ d' \ \ldots\ldots \text{ inverted} \end{matrix} \right\}$.

It is evident that by measuring the resistance between each of the wires (a, b, c, and d), in turn, with two different current-strengths, we have data which enable us to calculate the mean resistances of the junctions as well as that of the selenium. If, with the two different current-strengths, the calculated values of the junctions agree, we may assume that the change resides in the selenium; on the other hand, if the selenium agrees in both calculations, the change must reside in the junctions.

The above tests furnish the following results :—

Resistance of	Measured.	Current = 2·9 microw.	Current = 0·42 microw.
		meg.	meg.
$b + b'$	{ direct	·0945	·1022
	{ inverted	·0942	·1021
$c + c'$	{ direct	·1756	·1777
	{ inverted	·1746	·1775
$s + s'$	......	·5028	·5032

The agreement is in favour of the selenium, the mean resistance of which does not appear to change by decreasing the current-strength : the mean resistances of the junctions, however, increased, one of them considerably ; and it is therefore probable that in them, and not in the selenium, lies the change in question.

Resistance altering with strength of Current.—Professor W. G. Adams has pointed out that when the battery-power is increased the apparent resistance of the selenium is diminished. In some of the specimens of selenium which I tested, I found that while the current was weak, up to a certain limit, the resistance *increased* in one direction and *decreased* in the other direction ; but after passing the limit in question, the resistance *decreased* in both directions for any further increase of current.

One plate of selenium, annealed at 200° C., was kept at a constant temperature and measured in a Wheatstone bridge, the proportional resistances of which were respectively 1000 and 100,000 ohms. The selenium was inverted each time by a commutator, so that measurements were repeated in both directions, whilst the currents in the bridge-resistances always went in the same direction.

Current in selenium.	Measured resistance.	
	Current direct.	Current inverted.
microwebers.	megohm.	megohm.
2	·4107	·4093
4	·4119	·4080
6	·4128	·4072
8	·4131	·4063
10	·4133	·4056
12	·4133	·4050
14	·4133	·4047
20	·4130	·4038
30	·4126	·4023

A similar result was found with a second plate, the resist-

ance of which was smaller.   The following are the observations:—

Current in selenium.	Measured resistance.	
	Current direct.	Current inverted.
microwebers.	ohms.	ohms.
0·6	40090	40130
3·0	40020	40300
6·0	39800	40400
9·0	39600	40440
14·4	39240	40450
21·6	38800	40370
50·4	37570	39790
72·0	36900	39350
108·0	36130	38720

In each case it was found that the resistance of the selenium had somewhat increased during the measurement, due probably to heat generated by the current.

The behaviour of the selenium in these experiments is open to one of two interpretations.   If due to resistance, the alteration is possibly a consequence of the operation of the Peltier effect ; if due to electromotive force, it is probably a simple consequence of electrolytic polarization.

When the current passes through the selenium, it encounters a compound conductor analogous to that in which Peltier observed the cooling and heating effects of the junctions between antimony and bismuth.   The crystals round the point of contact which cools contract, and have a tendency therefore to recede and to make the contact with their neighbours still worse ; the crystals about the heated contact expand and improve the conduction.   The effects are opposite, but not necessarily equal, because one junction may be more susceptible than the other.   It is of course questionable how far this heating and cooling of an uncertain number of points of bad contact between the crystals of a selenium plate near the electrode would be sufficient to produce a sensible effect. But it is necessary to bear in mind the nature of the doubtful contacts which we are probably dealing with, and which the smallest conceivable approach or recession may improve immensely or break entirely.

When the direction of the current in a compound conductor is reversed, the heat or cold produced at one of the junctions has to disappear, and the opposite effect to be developed, before the resulting change can be completely observed.   It was found that when the bridge resistance was adjusted, in anti-

cipation of a changed (increased or decreased) resistance, before inverting the selenium, the galvanometer (introduced immediately after the inversion) showed a deflection always indicating that the resistance had not entirely changed but had still a value in the direction of that last measured; and it required two or three minutes to arrive at the settled value corresponding to the new conditions. This difference was small but distinct, and would be such as would correspond with change from heat to cold or *vice versâ*.

On removing the battery and inserting a galvanometer, the discharge current which issues from the selenium is in the opposite direction to the battery current, and agrees with the thermoelectric current which would result from the Peltier effect.

On the other hand, the supposition that the behaviour is due to polarization is the more probable. The increase of the resistance with increasing current (when weak) in one direction, indicates the existence in the selenium of a small independent electromotive force, and leads to the suspicion that a portion of the material in contact with one of the platinum electrodes is in an electrolytic condition, or both perhaps, one being more so than the other. As the measuring current increases in strength, it appears to polarize the electrodes in the selenium, as in an ordinary electrolytic conductor, and the small independent current is overpowered and lost sight of. The apparent decrement of resistance by increasing the battery, is probably due to the fact that the polarization increases in a less ratio than the measuring current, so that when this current is weak the polarization is proportionally stronger, and the apparent resistance higher, than when the measuring current is strong. The discharge after removing the battery is such as would answer to the depolarization of an electrolyte.

Electromotive force of crystalline Selenium.—The action of light in modifying the conductivity of selenium is evidently a surface action, the effect of which penetrates very little, if at all, into the mass. It therefore occurred to me that the phenomenon could best be studied, particularly in relation to heat, by making up the selenium plate in the form of a galvanic element. By this means we can deal with the surface without reference to the interior, both as regards light and heat.

A plate of crystalline selenium was prepared at 200° C. with a platinum wire fused into it, by which it was suspended in a test-tube. The back of the plate and the platinum wire near it were covered with a black insulating varnish. The tube was then placed in a light-tight box, in which a shutter could

be opened to admit light to the uncovered face of the selenium. A pole of platinum foil was placed in the tube, and distilled water sufficient to nearly cover the selenium plate. This selenium (galvanic) element, when in the dark, gave an electromotive force of 0·112 volt, the selenium being positive to the platinum. On admitting diffused daylight, the direction of the current was changed, the selenium becoming negative to the platinum, with an electromotive force of 0·056 volt; so that by the admission of diffused daylight the selenium surface had become very much less electropositive than it was in the dark.

Two similar plates of selenium were prepared and placed side by side in a suitable cell, which was enclosed in a light-tight case with two shutters, by means of which light could be admitted to one or the other of the plates at pleasure. Distilled water being poured into the bottom of the cell, so as to reach about three fourths up each plate, the electromotive force between them was measured with an accumulator, discharge-key, and galvanometer.

Both plates in the dark gave a very slight current. Then light was admitted by one of the shutters being opened. The plate on which the light fell instantly became electronegative. The consecutive readings were :—

$$-0·05 \text{ volt}$$
$$-0·04 \text{ ,,}$$
$$-0·03 \text{ ,,}$$

Then that shutter was closed and, after a few minutes, the other opened. The readings were now :—

$$+0·09 \text{ volt}$$
$$+0·08 \text{ ,,}$$
$$+0·07 \text{ ,,}$$

On connecting a galvanometer direct between the poles and observing the deflection, it was found that the current immediately following each reversal was higher, and that it subsided to a lower reading in a short time. This is probably due to polarization of the plates, and is exactly what might be expected.

Action of Light and Heat the same.—The next experiment was to ascertain if the effect of light upon the surface is the same or the reverse of that of heat. Two short lengths of glass tube, $a\,a$ and $a'\,a'$, fig. 4, were stopped at the lower ends with corks, b and b', through which strips of platinum foil were passed. Across the tops of the vertical tubes

(which were nearly filled with distilled water) two smaller horizontal glass tubes, *c* and *c'*, were fixed, over each of which a small saddle of white blotting-paper, intended to act as a conducting pad, was laid. The ends of the blotting-paper dipped into the water, and formed the connexion with a plate of crystalline selenium, *s s'*, which rested on the top. The circuit was completed by inserting a galvanometer (G) between the strips of platinum foil. The whole was mounted in a light-tight case with a diaphragm, *d d'*, so that light could be excluded from or admitted to either side at pleasure. When light was admitted to one side, it had of course to reach the contact surface of the selenium, after passing through the glass, the water, and the blotting-paper pad. It was therefore diffused and much weaker than had it fallen directly upon the face of the selenium as in the previous experiments. The indications were therefore less in amount, but nevertheless perfectly distinct.

Fig 4.
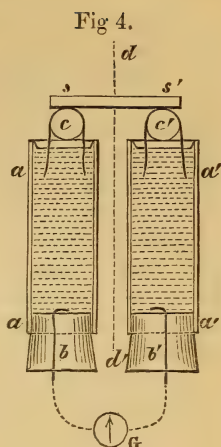
Next, instead of admitting light to the selenium plate, both of the tubes were kept dark, and warm water was passed through either *c* or *c'*, so that the wet blotting-paper pad upon it, and therefore the contact face of the selenium and water, was slightly warmed. A current was immediately observed. When this current had subsided, warm water was passed through the other tube, and the new deflection noted.

Now in both these tests it is evident that we are dealing, in respect of light and heat, with the *surface* of the selenium only, and have therefore eliminated any effects due to the molecular condition of the interior.

Lastly, the plate of selenium was removed, and a small bar of copper and zinc, soldered together in the middle, was laid across the pads, for the purpose of determining the direction in which heat and light had acted in modifying the electro-positiveness of the selenium. The following are the results :—

	End *c*.	End *c'*.	Deflection.
(1)	illuminated	dark	−200 div.
(2)	warmed	cold	−160 ,,
(3)	copper	zinc	− off scale.
(4)	dark	illuminated	+ 50 div.
(5)	cold	warmed	+ 80 ,,
(6)	zinc	copper	+ off scale.

Therefore the effects of light and heat upon the surface of crystalline selenium are identical. Both heat and light render the contact-surface between crystalline selenium and water more electronegative; and therefore we may surmise (although the experiment does not amount to a proof) that the surface does not become more metallic, as has been assumed as accounting for the higher conductivity of selenium in the light.

It is worthy of remark, that the end which was the more sensitive to light was also more thermoelectric. The difference may be due to inequality in the selenium plate, one end being better annealed than the other; and it indicates still further the similar behaviour of heat and light in this experiment.

Effect of Light on Conductivity.—The object of the following experiments was to determine whether the effect of light upon a plate of selenium when in a galvanic circuit, in increasing the current, is due to a photo-electromotive force in the same direction as the battery current, or to a decrement of resistance.

Let the resistance between the wires of the selenium plate in the dark and of the galvanometer be r, the electromotive force of the measuring battery be E, and the observed current c. On admitting light the current increases to c', and one of two things must have happened. Either (1) the increment of current is due to a decrement (x) of resistance, in which case

$$c = \frac{E}{r} \text{ has changed to } c' = \frac{E}{r-x},$$

$$x = E \frac{c'-c}{cc'}; \quad . \quad . \quad . \quad . \quad . \quad \text{(I.)}$$

or (2) the increment of current is due to a photo-electromotive force (y) in the selenium, in which case

$$c = \frac{E}{r} \text{ has changed to } c' = \frac{E+y}{r},$$

$$y = E \frac{c'-c}{c}. \quad . \quad . \quad . \quad . \quad . \quad \text{(II.)}$$

With a single measurement it is, of course, impossible to discriminate between the two cases; but by a known augmentation of battery and of resistance, it is easy to find which supposition affords the better agreement.

I increased E to 2E, and inserted a known resistance (r_1) as nearly equal to r as I could make it. The resulting current, when the selenium was in the dark, was c_1; and it increased to c_2 when subjected to the same source of light as

before. Then either

$$c_1 = \frac{2E}{r + r_1} \text{ changes to } c_2 = \frac{2E}{r + r_1 - x},$$

$$x = 2E \frac{c_2 - c_1}{c_2 c_1}; \quad \cdots \quad \text{(III.)}$$

or

$$c_1 \text{ changes to } c_2 = \frac{2E + y}{r + r_1},$$

$$y = 2E \frac{c_2 - c_1}{c_1}. \quad \cdots \quad \text{(IV.)}$$

It is plain that, if the values of x calculated by (I.) and (III.) agree better than those of y calculated by (II.) and (IV.), the change must be due to resistance.

The battery consisted of twelve similar Daniell's cells of large surface. They were connected with a commutator, by which, with a single movement, they could be connected up either in series or six and six parallel. In this way, by using the same elements in each measurement, the effect of any slight accidental difference in their electromotive forces would be, to a great extent, neutralized. No difference could be observed by the discharge of an accumulator; and therefore I assume they were in every respect equal. The currents were measured by means of a reflecting galvanometer.

A selenium plate with six elements in the dark gave a current $c = 0.498$ microweber. When the battery was doubled and the constant resistance r_1 added, the current observed was $c_1 = 0.508$ microweber.

Diffused daylight admitted to the selenium, with the six double surface elements the current increased to $c' = 0.860$ microweber; with the twelve elements in series and resistance r_1 the current was $c_2 = 0.643$ microweber.

Putting $E = 6.72$ volts, the values of x and y, calculated from the above data, are as follows:—

x		y	
I.	III.	II.	IV.
5·68 meg.	5·56 meg.	4·89 volts.	3·57 volts.

Similar measurements were made with two other plates, which gave the following results:—

x		y	
I.	III.	II.	IV.
6·39 meg.	6·43 meg.	5·76 volt.	3·86 volt.
55·2 ,,	53·0 ,,	0·75 ,,	0·53 ,,

It appears from the above, that the agreement between the calculated values representing the change, on the supposition that it is due to a decrement of resistance, is much greater than that between the values calculated on the supposition that it is due to an electromotive force set up in the selenium in the same direction as the battery current.

There is much experimental work to be done yet with selenium before any theory of its behaviour can be advanced with confidence. So far, the experiments seem to suggest the suspicion that light causes a modification of the surface tension of selenium, possibly an expansion of the crystalline surface. The superficial crystals expanding and pushing against each other, so as to improve the doubtful points of contact previously existing between them, may account for the observed increase of conductivity in the light. Such a superficial expansion would probably be occasioned by heat; and this heat on the extreme surface might account for the alteration, by light, of the potential of the selenium plate, when made up in the form of a galvanic cell, being in the same direction as the alteration of potential produced by the direct application of heat when the selenium is in the dark. It might also account for the decreasing sensibility of the selenium by continued exposure to light, the superficial heat penetrating into the interior and relieving the state of tension of the surface.

I apprehend that the superficial atoms of any body, which are bounded on one side only by similar atoms, and on the other side by the medium in which the body is immersed, may be capable of assuming vibrations of different periods to those which the atoms underneath the surface can assume. It may also be that the luminous rays striking upon the superficial molecules of selenium, impart a vibration to them of a slower period than those of the exciting waves, and which corresponds nearer to the period of the heat rays.

To return, however, to the object with which this inquiry was undertaken, viz. the production of constant resistances for measuring-purposes, it is evident that selenium is, from its peculiar nature, a very unsuitable material. In its amorphous state it is dielectric; and in its imperfectly crystalline state its character seems to be that of a dielectric more or less charged with conducting crystals.

This character it probably never entirely loses, even when crystallized as far as it can be; and to this fact is probably due, in a great measure, its peculiar behaviour.

In the light it would of course be utterly useless for measuring-purposes, whilst in the dark the apparent resistance of its junctions with the conducting wires changes, not

only with the direction of the current, but likewise with its strength, and to some extent also with its duration. To construct a selenium resistance for exact measuring-purposes, coefficients for all these changes would have to be determined, at very considerable trouble; and when determined, the inconstancy of the material is such that they would probably soon be altogether inapplicable.

In preparing the apparatus and making the experiments, I have been greatly indebted to the efficient aid rendered me by Mr. McEniry.

Grosmont House, Hampton Wick,
May 1, 1878.

LVIII. *On the Edge-angle and Spread of Liquids on Solid Bodies.* By G. QUINCKE.

[Concluded from p. 339.]

10. $SPREAD$ *of Liquids upon the Surface of Solid Bodies.*
—It is possible to form an opinion in another manner than by direct measurements, about the magnitude of the surface-tension at the boundary of a fluid and of a solid body, from the magnitude of the edge-angle which a fluid-surface forms with a solid body.

From equation (5) follows at once

$$\alpha_{21} > \alpha_{13},$$

if the edge-angle θ of the common bounding-surface of the fluids 2 and 3 with the solid body 1 is an acute angle for fluid 3.

For glass as the solid body, and water as fluid 3, and for bisulphide of carbon, chloroform, olive-oil, turpentine, petroleum, or mercury as fluid 2, this condition is fulfilled with the slight allowances already explained* for impurity of the solid surface of glass.

The water will therefore have a greater adhesion to glass than the fluids named.

Whenever free fluid-surfaces bounded by air are absent, none of the fluids 2 investigated drives the water away from the glass surface, however different in magnitude may be their capillary constants. The edge angle was only in rare cases $0°$; consequently the water also usually did not drive the other fluids from the glass surface.

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* Compare the researches upon flat drops and bubbles, Pogg. *Ann.* cxxxix. pp. 18–20, 22 (1870); upon submerged capillary tubes, *ibid.* pp. 42–44; upon the ascent in capillary tubes of several superposed fluids, *ibid.* pp. 50–52. And in Phil. Mag. April, May, and June, 1871.

That the phenomena produced by the same liquids may change with the nature of the solid substance follows from the researches of Chevreul*, in which, when air was excluded, olive-oil was driven away by water in porous porcelain, while in porous whitelead water was driven away by olive-oil.

This circumstance is therefore worthy of notice, since it has often been said that fluids with a lesser capillary constant, or tension of the free surface, drove away from solid bodies the fluids with a greater capillary constant of the free surface. If the fluids are brought into contact with the air, besides being brought into contact with one another and with the solid substance, then indeed the water is driven away by most of the fluids 2 with which the researches recited above were conducted; and I have already previously completely discussed † the cause of this phenomenon. Hence it follows that the contact of air with fluids 2 and 3, which are in contact with a solid body 1, may promote or initiate the driving away of fluid 3 by fluid 2 from the surface of a solid body, when once $\alpha_2 < \alpha_3$, and in consequence the sum of the surface-tensions a minimum ‡.

This remark appears to me to be of importance for the comprehension of the influence of the air, or of gases generally, upon the processes of diffusion in the nourishment of plants and animals, and in the influence upon the digestion of drinks containing carbonic acid.

If two fluids, which are mutually miscible in all proportions, come into contact with the same solid body (with glass in the present case) simultaneously, *without access of air*, there can be no surface-tension at the surface of contact of the two fluids, and the fluid of the lesser surface-tension α_{12} of the common boundary with the glass must drive away from the solid body the fluid with the greater surface-tension.

According to the figures of Tables VIII. and IX., water must therefore drive away alcohol from a surface of glass. This is in harmony with experience, since burnt clay and quartz-sand, which behave similarly to glass, deprive aqueous alcohol of water, as Wagenmann and I have found §.

According to Table IX. turpentine must drive olive-oil

* *Comptes Rendus*, lxiii. p. 63 (1866).

† Pogg. *Ann.* cxxxix. p. 58 (1870); and Phil. Mag. June 1871, pp. 466–9.

‡ Pogg. *Ann.* cxxxix. p. 61 (1870); and Phil. Mag. June 1871, p. 471.

§ Pogg. *Ann.* cx. p. 61 (1860). Compare also W. Schmidt, Pogg. *Ann.* xcix. p. 370 (1856), and Duclaux, *Ann. de Chim. et Phys.* (4) xxv. p. 486 (1875).

away from a glass surface; the contrary must occur according to Table VIII.

In fact the first occurs, since, as I formerly[*] proved by the change of height of capillary ascent, in a capillary tube filled with olive-oil and dipped in turpentine, the latter drives away the olive-oil, and finally spreads upon the surface of the olive-oil.

In the case of all aqueous saline solutions which are miscible with water in all proportions, the saline solution must drive away the water from the surface of the glass the more easily the more concentrated it is, since (α) increases[†] with augmented concentration, and, as is shown in detail in § 9, α_{12} is the smaller as $(\alpha) = \alpha_2 \cos \theta$ is greater for the free surface of the liquid.

The same must also occur in the case of different saline solutions which are miscible in all proportions, and which exercise no chemical action upon one another (as giving precipitates &c.).

Since for all saline solutions the edge-angle against glass has nearly identical values, the saline solution with the greater cohesion (α) will drive away from the surface of the glass that with lesser cohesion.

Hence it follows, further, that from a dilute saline solution, as a mixture of water of less cohesion with concentrated saline solution of a greater, there must collect on the surface of the glass a concentrated solution. The most soluble substances, which in strong concentration exhibit the greatest cohesion, must also collect specially readily on the surface of glass, or will be specially strongly absorbed by the glass surface.

The phenomena of so-called selective absorption appear to confirm this. Quartz, porcelain, &c., which have for saline solutions an edge-angle similar to that of glass (compare § 7 above), appear to absorb carbonate of potash, chloride of calcium, chloride of magnesium, &c. specially strongly, and hence, therefore, those salts which with strong concentration exhibit the greatest cohesion (α).

This is in harmony with experience in the fact that new, unused porcelain cylinders (such as are usually employed in galvanic batteries), when they have once been in contact with a dilute saline solution, retain salt absorbed even after a long-protracted steeping in water.

The influence of the molecular nature of the solid substance exhibits itself specially clearly in the case of lightly and

[*] Pogg. *Ann.* cxxxix. p. 55 (1870).
[†] Ibid. clx. p. 371 (1877).

strongly ignited carbon; the former exhibits, according to the researches of Graham *, a very strong selective absorption, the latter an absorption scarcely perceptible.

On account of the difficult mobility of saline solutions on the surface of glass, and the residual attraction of water to a concentrated saline solution (here disregarded), the concentrated saline solution may certainly become partially dissolved away and removed from the surface of the glass; and this solution leads directly to a theory of diffusion at the surface of solid bodies.

11. Brücke † has founded his theory of the diffusion of a liquid at the surface of solids upon researches with turpentine and cotton-seed oil, which were brought into contact with one another in a space bounded by nearly-adjacent surfaces of glass.

According to Brücke the turpentine drives the cotton-seed oil from the glass surface. The liquid filling the capillary space may be divided into three films, of which the middle one consists of turpentine and cotton-seed oil, and the surface layers of turpentine. Whilst from the turpentine side of the middle layer cotton-seed oil is continually withdrawn, and turpentine continually from the cotton-seed-oil side, since the turpentine of the surface-film is attracted more strongly by the cotton-seed oil than by the turpentine in the vessel, some of this continually wanders over into the cotton-seed oil, and the volume of the latter is augmented.

This theory of the diffusion of turpentine and cotton-seed oil upon a glass surface is deduced from the supposition that the turpentine drives away the cotton-seed oil from the surface of glass. Brücke founds this supposition upon the experiment that a small drop of cotton-seed oil placed upon a clean glass plate is driven away by a neighbouring drop of turpentine. The attraction of glass to turpentine is too great, relatively to that of turpentine to itself, for the contact-angle between these two substances to approach 180°.

But where the turpentine meets the cotton-seed oil it drives it away from itself by virtue of its greater adhesion.

So, consequently, the experiment just described does not prove this supposition, since in it the liquids, besides being in contact with one another and with glass, are also in contact with air. Nevertheless the supposition laid down as the foundation of the theory is right, as my researches mentioned above (§10) show, where, without access of air, the

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* Pogg. *Ann.* xix. p. 139 (1830).
† Ibid. lviii. p. 82 (1843).

turpentine drove the cotton-seed oil from the surface of glass. Brücke's theory of the diffusion of liquids at the surface of solid bodies satisfies every postulate when once the spread of one of the liquids upon the surface of the solid has been explained.

As already mentioned above, *a liquid* 2 *with a small capillary tension* α_{12} *at the boundary of a solid body* 1, *must drive away a liquid* 3 *possessing a greater capillary tension* α_{13} *at the boundary of the same solid body, provided access of the air is prevented and that the liquids* 2 *and* 3 *are miscible in all proportions.* In this case $\alpha_{23} = 0$, and equation (6 A) is immediately fulfilled.

Liquids which are miscible in all proportions may also be arranged in relation to any given solid body 1 in a determinate series according to the magnitude of the capillary constant or surface-tension of the common boundary of the solid body and of the liquid in question. Each liquid will be driven away by one standing lower in this series. But the series changes with the nature of the solid body.

I have detailed above (§ 9, conclusion) the difficulties which beset an exact determination of the magnitudes α_{12}, α_{13}, &c.

In view of the uncertainty of inferring the surface-tension of the boundary of liquids and solids from observations on flat drops, the method of observing directly the spread of a liquid on the surface of a solid body when air is excluded deserves at least the preference.

The method employed by me in the case of olive-oil and turpentine cannot unfortunately be applied to all liquids, and fails where no diminution of the capillary height of ascent can be observed, and when the liquid with the greater capillary constant of the free surface is such that it spreads upon the surface of the solid. This is, for example, the case with water and alcohol upon the surface of glass.

The influence upon the diffusing liquids of the dependence of diffusion upon the nature of the solid body is beautifully shown in the case of alcohol and water which are separated by a piece of pig's bladder or by india-rubber tissue. In the first case the volume of the water diminishes ; in the second it increases.

Here Brücke [*] has already shown, by experiment, that on the exclusion of air the water overspreads the surface of the bladder, and the alcohol invests the surface of the indiarubber tissue—that therefore in the first case the water can flow to the alcohol, in the second the alcohol can flow to the water.

* Pogg. *Ann.* lviii. p. 87 (1843).

Such a porous partition behaves similarly to a more or less completely closed valve, and permits, according to its nature or chemical properties, the liquid 2 to flow towards liquid 3, or liquid 3 towards liquid 2.

The experiments mentioned show that

$$\alpha \text{ (water, bladder)} \quad < \alpha \text{ (alcohol, bladder)}$$
$$\alpha \text{ (water, indiarubber)} > \alpha \text{ (alcohol, indiarubber)}.$$

The surface-tension at the boundary of a liquid and a solid body appears, like the surface-tension at the boundary of two liquids, to be less in proportion as one liquid is the more able to dissolve the other body.

Besides, it is in most cases very difficult, in single experiments, to distinguish the *surface*-diffusion through the interposition of the solid partition from the *free* diffusion which occurs between two liquids without the interposition of the solid partition. That is also the reason why these processes have hitherto been so little investigated.

Since the friction of the liquid particles against one another decreases with augmenting temperature, with an augmenting temperature the diffusion must increase for equal differences between the magnitudes α_{12} and α_{13}, where again the solid is denoted by 1 and the diffusing liquids by 2 and 3. And experience generally* agrees therewith.

12. *Imperceptibly thin films of liquid. Creeping of salts.*—Many times already have I drawn attention to the want of harmony between theory and the observed facts, and at the conclusion of § 9 have inferred therefrom that imperceptibly thin films of a foreign substance upon the surface of the solid body may be the cause of this accidental circumstance.

On account of the numerous observations, and the accuracy of the methods employed, it is specially remarkable that in the measurements upon air-bubbles in aqueous saline solutions an edge-angle of from 20° to 30° is generally found, and yet by the direct method of reflexion a much smaller value is found.

It implies this, according to my thinking :—that clean liquids spread instantly upon the *clean* surface of solid bodies, just as clean fluids spread instantly upon the *clean* surface of mercury ; and that if no spread occur, and the edge-angle differ from 0°, the surface of the solid body, like that of the mercury, is always coated with an excessively thin film of a foreign substance which hinders the spread.

This thin film (which itself escapes observation, because it

* Brücke, Pogg. *Ann.* lviii. p. 78 (1843).

is so thin that it can only be perceived in solitary cases with difficulty and by optical aids) may itself consist of the liquid whose drops have been placed upon the surface of the solid body.

As a drop of oil, placed upon a clean surface of water, divides itself when sufficiently large into two parts, of which one part is drawn in a very thin film over the clean water-surface, whose capillary constant or surface-tension is diminished, thereby rendering it possible that the rest of the oil may remain lying in a lenticular globule* upon the modified surface of the water, so when water or aqueous saline solutions are placed upon the clean surface of a solid body, a portion of the liquid spreads out in an excessively thin film upon the solid surface. The original surface-tension (if we retain the expression on account of the analogy with clean surfaces of liquid) of the clean solid surface is thereby diminished ; the remainder of the liquid stays upon the modified solid surface in a lenticular form, and with an edge-angle $> 0°$.

This film of foreign liquid forming over the surface of the solid body at the first moment with *very great rapidity*, is of a thickness varying with the temperature and purity of the surface, and is usually thinner than the double radius $2l$ of the sphere of sensible action, or greater than 0·000050 millim. But according to the rapidity of the spread or of the formation this film has different thicknesses and different properties, and modifies the tension of the previously clean surface of the solid substance in different manners also. The change of density which the liquid in the immediate neighbourhood of the solid surface experiences must depend upon the time during which the molecular forces of adhesion act upon these liquid particles.

Upon a liquid particle at a distance $< l$ from the solid wall there acts the difference of the molecular forces which are exerted by the substance of the solid wall in one direction, and by the substance of the liquid and air lying on the other side of it in the opposite direction.

The action of the air is negligibly small; and one may therefore say that upon the liquid particles in the immediate neighbourhood of the solid wall there is exerted a greater molecular force, and that the change of density thereby produced is greater, in proportion as the film of liquid which lies over it is thinner. The change of density will be different at different distances from the solid wall, and at given points

* Pogg. *Ann.* cxxxix. p. 76 (1870).

of the liquid will be the greater as the whole film of liquid which covers the solid body is thinner.

But since the change of density is propagated from particle to particle, it may very well be observable, even at a distance, $> l$, or greater than pl, where p may signify a factor greater than 2.

These changes of the density and molecular properties of the liquid, which in the so-called imperceptibly thick films that overspread the surfaces of solid bodies, play a considerable part in all phenomena concerning the attraction of fluids to other fluids or solid bodies, and make the investigation of these phenomena the more difficult, since we know very little or nothing of the magnitude and nature of the changes, and consequently cannot bring them into our calculations.

The phenomenon not theoretically explicable, that a drop of oil upon a water-surface covered with a thin oil-film, or a drop of water upon a glass-surface covered with a thin water-film remains in a lenticular form, depends probably upon some such modification of the liquid in the thin liquid film.

Whether the difficult mobility of a lenticular drop of liquid on a level solid surface (compare § 4) is also determined by the presence of an imperceptibly thin film of liquid, as I formerly * observed in the case of liquid surfaces, or whether the difficult mobility of the solid substratum is also involved, cannot be decided with certainty off-hand. The fact that upon the cleanest possible solid surfaces the liquids can spread with remarkable rapidity points to the former suggestion.

These thin films of liquid, modified by the proximity of a heterogeneous substance, play a great part in nature; and there is certainly no doubt that the life of organic nature depends principally upon them.

That these films, whose thickness in many cases amounts only to a small fraction of the mean length of a wave of light, cannot be seen, is a physical difficulty for investigation, but no proof against their existence. In the investigation of the common surface of different liquids I have already remarked † upon this circumstance, and hinted at the difficulties involved in the investigation.

But it is possible to prove the existence of these thin films in another way than by the edge-angle at the boundary of a drop of liquid on a level solid body. Newton's rings were

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* Pogg. *Ann.* cxxxix. p. 71 (1870). Compare also Marangoni, *Cimento,* v. p. 239 (1872) ; *Berol. Ber.* xxviii. p. 184 (1872).

† Pogg. *Ann.* cxxxix. pp. 37, 39, 69, 73–76 (1870) ; and Phil. Mag. [IV.] vol. xli. (1871).

exhibited by aqueous solutions of carbonates of soda and potash on the glass plate under which the bubbles of air lay*.

But even in those cases where the film is so thin that interference-colours fail, there are formed, on the surface of the solid substance, near the sharply-defined flat drop of a saline solution, crystals of the salt employed, or, as the phenomenon has been well named, the salt *creeps*.

The creeping, or efflorescence, of the salts is usually explained † by the liquid which is drawn up between the wall of the vessel and the salt which has crystallized out. This explanation is legitimate when once the first crystals have formed. The formation of these first crystals, however, which often form upon the solid surface at a great distance from the liquid, is determined by the thin film of saline solution which overspreads all solid bodies (metals, glass, quartz, &c.) in imperceptible thickness. If a portion of the water evaporates, a fresh quantity of the saline solution streams into the interior of the thin film. The thickness of the film and the proportion of the inflowing saline solution are the greater as the surface of the solid body is the cleaner.

Temperature also appears to have an essential influence upon the rapidity of the liquid mixture flowing into the thin film.

The crystals of the salt form where the water evaporates most rapidly, at the outermost edge of the thin liquid film.

Frequently the surface of the solid body is unequally clean at different points; and then the crystals form first on the cleanest places, and at a greater distance from the flat drops of liquid than on the places that are less clean.

The creeping is the more striking as the solid surface is cleaner, or as the edge-angle at the boundary of the flat liquid drop is less (since this latter depends closely, as I have shown above, upon the former).

Creeping does not occur if the solid surface be covered with a thin film of oil. Since glass surfaces remain clean longer in the open air than metal surfaces, salts usually creep more readily upon the former than upon metal surfaces. Besides, it has long been known that the creeping of salts may be hindered by smearing a glass surface with grease.

Moreover those salts whose solutions in the requisite concentration are most mobile must exhibit creeping the most

* Pogg. *Ann.* clx. p. 369 (1877).
† Compare Barentin, *Handwörterbuch der Chemie und Physik,* p. 636 (1842).

plainly: hence liquids having minimum viscosity or maximum fluidity, must exhibit, *cæteris paribus*, the most considerable creeping. With this the facts appear, in general, to harmonize; for solutions of salammoniac, saltpetre, and potassium chloride, liquids possessing great fluidity*, exhibit the phenomenon of creeping to a special degree.

13. *Demonstration of thin Liquid-films by Electric Discharges.*—In addition to the methods of the edge-angle and of the phenomena of creeping, the existence of the thin liquid-film near the flat drops of liquid upon a solid surface may be proved by a third electrical method, provided the solid substance be an insulator (*e. g.* glass), and the liquid upon it be a conductor of electricity.

Two vertical platinum wires, P_1 and P_2 (Plate XII. fig. 1 *d*), of 0·138 millim. diameter, were ignited in a flame of pure alcohol, and placed at 8 millims. distance from one another upon a horizontal piece of plate glass of 60 millims. length and 40 millims. breadth, which again lay upon a larger horizontal piece of plate glass. At a certain time the wire P_1 was connected to a charged gold-leaf electroscope by a long thin silver wire; the other wire, P_2, leading to earth.

Each platinum wire was fastened with shellac to the corner of a triangle of plate-glass of 35 millims. width and 3 millims. thickness. Each formed, along with two similarly fastened plated copper wires, the leg of a little tripod 23 millims. high, whose weight pressed it lightly against the surface to be investigated.

With a seconds' watch, or a metronome beating half seconds, the time τ_0 was estimated which was required for the gold leaves 18 millims. long and 2 millims. broad, of an electroscope to fall together from an angular separation of 60°. The same experiment was then again repeated and the time τ of the discharge of the electroscope measured, after a flat drop of water or saline solution had been deposited near the platinum wires without touching them. Simultaneously the edge-angle θ was measured by the method of reflexion (see § 3).

It was found that τ_0 and τ were greater or less according as a longer or shorter time Z had elapsed since the cleansing of the plate glass. But τ was always much less than τ_0, and in general was only half as great as τ_0.

As examples I subjoin a series of such determinations, where opposite each solution is stated in brackets whether 1, or $\frac{1}{2}$, or $\frac{1}{4}$ volume of the concentrated solution was contained in 1 volume of the liquid employed.

* O. E. Meyer, Pogg. *Ann.* cxiii. p. 404 (1861); and Grotrian, Pogg. *Ann.* clvii. p. 243 (1876).

TABLE X.

Liquid.	Time since plate was cleansed. Z.	Time of discharge of the electroscope before τ_0	after τ.	Edge-angle. θ.
Black Glass. No. 1.				
Water..........................	0 h.	0·65	0·32	6 57
Solution of potassium chloride (¼)...	3	1·75	0·6	4 7
,, ,, ,, ...	24	42	22	21 55
,, ,, ,, (½)...	?	2·75	1	18 58
,, ,, ,, (1)...	0	1	0·4	4 32
,, ,, ,, (1)...	48	9	4	12 25
Black Glass. No. 2.				
Water..........................	0 h.	45	18	12 58
,, ...	1·5 h.	285	120	37 4
Salammoniac (1)	8 days	196	115	20 52
Plate Glass.				
Water...........	0 h.	6	4	3 16
,, ...	24	3·5	2·5	5 0
,, ...	48	1·55	0·2	4 52
,, ...	,,	1·65	1·05	7 44
,, ...	0	15	3·5	13 13
Saltpetre (¼)	12	2·5	1·5	13 13
,, (½)	0	18·5	13	22 43
,, ...	24	8	2·5	5 4
,, ...	8 days	50	11	34 42
,, (1)	0 h.	14	2	5 22
,, ...	14 days	200	32	29 0
Salammoniac (1)	?	52	27	27 12
Quartz.				
Water..........................	0 h.	19·5	12·5	36 29
,, ...	0	140	50	14 54 cleansed with a cloth.
Selenite.				
Water..........................	0 h.	7·5	3·75	6 47
,, ...	0	13	8·5	2 25
Saltpetre (1)	0	4	2·5	13 58
Magnesium chloride (1)	0	7	6	5 14
Salammoniac (1)	0	8·5	7	5 55

Table (*continued*).

Liquid.	Time since plate was cleansed. Z.	Time of discharge of the electroscope		Edge-angle. θ.
		before deposition of the drop. τ_0.	after deposition of the drop. τ.	
Mica (uniaxal).				
Water...........................	0 h.	1·75″	0·6″	2° 33″
Mica (biaxal).				
Water...........................	0 h.	3	1·75	2 15
Saltpetre (1)	0	2·25	1	

These experiments show that, other circumstances being alike, the time of discharge of the electroscope was less as the edge-angle was smaller or the surface of the solid substance cleaner.

Since the times of discharge for the same liquid were different in the different experiments, they cannot be due to the deposited drops alone, but must depend upon the state of the surface of the solid substance, which is modified in different ways by the substances deposited upon it.

The differences of time of discharge, however, are at once explained if it be admitted that a small portion of the liquid deposited spreads upon the surface of the solid substance in an extremely thin film imperceptible by optical methods, and that the remaining portion of the liquid rests with an edge-angle $> 0°$ upon the surface so modified or rendered impure by the liquid itself.

The edge-angle and the time of discharge were found to be less, according as the thin liquid film near the flat drop of liquid had a greater thickness, or as the surface of the solid substance was more completely clean.

That in the separate experiments the edge-angle and the time of discharge, or the thickness of the thin film of liquid near the flat drops, were found of different magnitudes cannot be surprising if we contemplate the difficulties which stand in the way of a complete cleansing of the plate, or if we consider how the gradually augmenting impurity of the surface is the result of accidentally settling particles of dust, the presence of which cannot be avoided, and yet whose influence cannot be allowed for.

14. *Influence of the velocity of the falling drops upon the thickness of the thin films of liquid.*

The time of discharge of the electroscope appears to vary, amongst other conditions, according as the drops have formed swiftly or slowly.

On two siphons of equal diameter (1·006 millim.) (see § 4) there were formed respectively 10 drops and 40 drops of water in a minute, of 19 mgrms. and 19·67 mgrms. weight each ; so that in one minute about 3·28 square centims. and 14·15 square centims. of free surface of liquid were respectively formed per minute.

As before, the times of discharge τ_0 and τ of the gold-leaf electroscope were determined for a distance of 8 millims. between the platinum electrodes, without and with drops of water, upon a clean glass plate. The edge-angle was also measured at the same time, according as the drops were formed slowly or rapidly.

Z signifies the time which had elapsed since the cleansing of the glass plate.

TABLE XI.

Water—Plate Glass.

	Slowly.			Rapidly.	
Z.	Time of discharge before | after deposition of drop.		Edge-angle. θ.	Time of discharge τ.	Edge-angle. θ.
	τ_0.	τ.			
0 h.	3″	2″	5° 34′	0·82″	3° 20′
0 h.	1·6	1	7 36	0·1	7 15
8 days	14	7·5	8 10	8·5	7 20
8 „	9·5	6·0	9 0	6·5	8 11

Whence it appears that a rapidly formed drop of water spreads upon a freshly cleansed glass plate in a thicker film than a slowly formed drop of water. The difference is imperceptible in old surfaces of glass cleansed some time previously.

The edge-angle appears to be less for rapidly-formed than for slowly-formed drops.

In the case of aqueous solutions of calcium chloride ($\sigma = 1·1639$), potassium carbonate ($\sigma = 1·4444$), and concentrated sulphuric acid, I have not been able to prove with certainty this difference of time of discharge between rapidly and slowly formed drops. In the case of the solutions of potassium carbonate and concentrated sulphuric acid, τ_0 was found greater than τ ; hence the time of discharge of the

electroscope was increased by the deposited drop of liquid. This would indicate that a thin film of liquid already previously existing upon the glass (probably moisture from the atmosphere) was driven away by the deposited drop of solution and repelled from the surface of the glass.

15. *Breath-figures.*—Finally a series of long-known phenomena, which I have included in a treatise * " On the Condensation of Gases and Vapours on the Surface of Solid Bodies," find their natural explanation in the influence which imperceptibly thin films of liquid on the surface of a solid body exercise upon the spread and edge-angle of a liquid.

The edge-angle which lenticular drops of water, alcohol, or other liquids make with the surface of a liquid or solid body differs in magnitude with the substance, and the thickness of the imperceptibly thin film with which the surface is covered. I established this formerly † for mercury and other liquids, and have now proved it for solid substances such as glass, mica, silver, &c., in the earlier part of this communication (§§ 4–7).

Suppose the thickness of this film of substance 2 upon the solid body 1 to increase from 0 to a maximum value D, then the value of the edge-angle will vary continuously from the value θ_1, corresponding to the clean surface of the substance 1, up to the value θ_2, corresponding to the clean surface of the liquid 2, and will then remain constant for any further increase of the thickness.

This maximum value D of the thickness, from which onwards the edge-angle acquires the constant magnitude θ_2, must in general depend upon the radius of the sphere of sensible action of the molecular forces. If no change of density occur in the interior of the substance 2 (as was amply set forth above in § 12), then of necessity $D = l$.

This last is the case if the substance 2 consist not of liquid, but of some solid body whose particles admit only of a slowly occurring shifting, so that the density may be regarded as constant during the period of the experiment. This relation has indeed been employed by me ‡ for a determination of the magnitude l; and at the same time it was proved that with an increasing thickness of the solid substance 2 the edge-angle did in fact approach a constant value θ_2. By this means, the edge-angles formed by mercury against films of iodide

* Pogg. *Ann.* cviii. p. 339 (1859), " Ueber Verdichtung von Gasen und Dämpfen an der Oberfläche fester Körper."
† Pogg. *Ann.* cxxxix. p. 64 (1870) ; and Phil. Mag. June 1871.
‡ Pogg. *Ann.* cxxxvii. p. 402 (1869).

of silver of various thickness upon glass were, amongst others, measured.

The substance 2 of the thin film upon the surface of the solid body 1 may therefore consist, as experience teaches, either of solid or liquid.

That it may also consist of gaseous matter or condensed gas cannot be doubted, since I have completely established the influence of electrolytically separated films of gas on the bounding surface of mercury and water, or of various acids and saline solutions [*].

Consequently, the surface 1 may belong to a solid body or to a liquid, and the thin film may consist of solid, liquid, or gaseous matter.

If the vapour of a liquid 3 be allowed to deposit upon the cooler surface 1 of a body rendered impure by a thin film 2, the condensed drops possess an edge-angle varying according to the thickness of the film 2 ; the places where the edge-angles have different values reflect the light in different ways ; and thus the various thicknesses of the film 2 can be estimated indirectly.

When aqueous vapour settles upon a plate of glass or metal, we obtain a so-called *breath-figure*, such as have been investigated principally by Moser [†] and Waidele [‡]. If mercury be deposited upon a film of iodide or bromide of silver modified by light or exposure in a camera, we have a so-called *photographic image* (" light-figure ") or *Daguerreotype*, a breath-figure produced by vapour of mercury.

In order that the breath-figures may appear plainly, the thickness of the film 2 must be less than the maximum thickness D mentioned above.

For the production of the breath-figures it is therefore advantageous to employ the cleanest surface possible of the body 1. Surfaces of solids must therefore be freed as much as possible from the adhering film of substance 2 by polishing with alcohol, tripoli-powder, &c.

Waidele found that freshly polished surfaces of silver gave the breath-figures best when they condensed the moisture with a blue tint, and worse when with a brown tint. This blue or brown tint I observed on the thin film of water which spreads near a drop of water upon a freshly cleansed surface of glass or silver. In the former case a portion of the drop of water spreads in a thicker film upon the solid surface before the liquid on the outer edge of the film has disappeared by

[*] Pogg. *Ann.* cliii. p. 193 (1874).
[†] Ibid. lvi. p. 177 ; lvii. p. 1 (1842).
[‡] Ibid. lix. p. 255 (1843).

evaporation than in the latter case. In the former case the film exhibits a tint of higher order on Newton's scale than in the latter—blue of the 1st order corresponding to a thickness of air of about 0·0002 millim., brown of the 1st order corresponding to a thickness of about 0·0001 millim., on the supposition that the thinnest place of the film in reflected light must appear white).

A drop of alcohol deposited upon a plate, and which more readily spreads (compare § 6 above) into a thicker film, even with a less-pure surface exhibits, like water, by reflected light, the blue of the 1st order.

The greater or lesser purity, and the greater or lesser capacity of the solid surface to give breath-figures, may therefore be estimated by the greater or lesser readiness with which water spreads, or by the greater or lesser thickness of the film of water which forms on breathing.

To judge by the measurements communicated in §§ 4–8, in spite of all precautions we shall never attain to the point of giving a clean surface even for a short time only to a solid body, or of keeping such for a longer time.

And safe as it may be to affirm that upon the surface of a solid body 1 a thin film of foreign matter exists, or, as I will term it, *adheres*, it must be difficult to determine accurately the nature and chemical properties of this adherent film. At all events by the contact or close proximity of other solids or liquids (porous substances, polished stones, coins) the thickness of the adhering film may be diminished ; or it may be increased by the deposition of fresh matter. The places of different thickness are distinguished after breathing upon the surface by differing edge-angles, or different appearance in the breath-figure.

If a sheet of paper cut into a pattern be laid upon a moderately clean surface of glass, and the pattern so cut away be then breathed upon, and the moisture be again allowed to evaporate, the water vapour carries off with it a portion of the adhering film of liquid or gaseous matter, the thickness of the film becomes less, and, on repeating the breathing after the removal of the sheet of paper, the edge-angle on the parts lying underneath the cut-away pattern will be less. The pattern will be visible in the breath-figure.

Electric currents are, as I have shown in another place [*], specially endowed with the property of setting in motion liquids at the surface of solid bodies, even such as were not previously movable. The moving force is, *cæteris paribus*, the greater as the tension of the current is greater. It cannot,

* Pogg. *Ann.* cxiii. pp. 514 & 592 (1861).

therefore, be surprising that the electric discharges meeting a surface of glass in greater or less intensity from the elevated or depressed parts of a coin, remove the adherent liquid or gaseous film more or less strongly. Add to this the heating action of the discharges passing across the film of air between the coin and the glass. The elevated and depressed parts must when again breathed on be distinguished in the breath-figure as places of greater or lesser edge-angle.

Still more readily do the electric breath-figures investigated by G. Karsten[*] and Riess[†] succeed, formed upon the cleanest possible surface of a recently melted plate of pitch.

These electrical breath-figures can be exhibited with vapour of mercury or of iodine[‡], instead of the moisture of the breath, since these must likewise form small lenticular drops of varying edge-angle.

If an electric spark has been discharged upon the surface of a plate of glass, mica, or metal, the adherent film will have been as good as completely removed from the places touched by the discharge. Then the water condensed on breathing will form lenticular drops no more, but will spread in a continuous film : the place hit by the spark appears glossy upon a dull ground and forms the *electric breath-figure* described by Riess.

———

I think the results of the foregoing investigations may be summed up as follows :—

1. The long-known properties of the bounding surface common to two fluids may be extended to the bounding surface common to a fluid and a solid body.

2. The common bounding surface of a solid body 1, and of a liquid 2, tends to become a minimum, or, as we may say, it is governed by a surface-tension α_{12} independent of the geometrical form of the surface, and dependent only upon the nature of the two substances 1 and 2.

3. The magnitude of the edge-angle of a solid body 1 and a liquid 2, which are both bounded by fluid 3, is determined only by the nature of the three substances, and is independent of the geometrical form of the surface.

4. The principal law of the capillary theory, given by Dr. Thomas Young, concerning the constancy of the edge-angle of the free surface of a solid body and of a liquid is a special case of the law expressed in summary 3, in which fluid 3 consists of air.

[*] Pogg. *Ann.* lvii. p. 493 (1842).
[†] Riess, *Reibungselectricität*, ii. p. 224.
[‡] Karsten, Pogg. *Ann.* lvii. p. 496 (1842).

5. The edge-angle may be determined indirectly by deductive calculation from the measurement of the form of flat drops and bubbles, or directly by reflection of light.

6. The edge-angle of the free surface of various liquids, such as water, alcohol, etc., and aqueous or alcoholic solutions of salts, upon clean surfaces of glass, crystals, or metals appears to be $0°$.   The liquids spread upon the clean solid surfaces.

7. If the edge-angle has, as is usual, greater values, then the solid surface has been covered with an (imperceptibly) thin film of a foreign substance, with the thickness of which the edge-angle alters.

8. The thickness of this thin film, however, cannot exceed a certain maximum value D, which is as great as, or greater than, the radius of the sphere of sensible action of the molecular forces.

9. This thin film adherent to the surface of the solid body may consist of solid, liquid, or gaseous matter.

10. It may also itself consist of the liquid deposited, and may be demonstrated, besides by the edge-angle, by the so-called " creeping " of salts, or by electric conduction on the surface of the solid body, and in single cases also by the interference-tints of the light reflected by it.

11. The imperceptibly thin films of the same liquid possess different properties according to the duration and the nature of their origin, or according to the nature of the solid body to' which they adhere.

12. Rapidly formed drops of water spread more readily upon freshly cleansed surfaces of glass than those formed slowly.

13. These imperceptibly thin films of foreign matter appear to be the cause of the deviations between theory and experience in the determination of the surface-tension at the common boundary of liquids and solid bodies.

14. When the edge-angle is $0°$, or has impossible values, there results a spread of the liquid upon the surface of the solid body.

15. In the case of liquids which are miscible in all proportions, the liquid with the less surface-tension α_{12} drives away that with the greater surface-tension α_{12}.   This surface-tension and the driving-away which may occur, change, however, with the nature of the solid substance.   This completes Brücke's theory of the surface-diffusion along a solid surface.

16. The presence of other fluids, and particularly of air, may essentially modify the spread of a liquid upon a solid surface.

17. The dependence of the edge-angle upon the thickness

of the imperceptibly thin film on the solid surface explains
the breath-figures obtained by Moser and Waidele with water
vapour, the photographic images (light-figures) of Daguerre
with mercury vapour, and the electric breath-figures of G.
Karsten and Riess with vapour of water, mercury, and iodine.

Heidelberg, June 30, 1877.

LIX. *On the Influence of Temperature on the Passage of Air
through Capillary Tubes.* By FRANCIS GUTHRIE, *LL.B.*[*]

IN using " Marsh's " apparatus for testing for arsenic, it
may be noticed that, when heat is applied to the exit-tube
to decompose the arsenuretted hydrogen, the liquid rises in
the tube which supplies the sulphuric acid, thus leading to the
supposition that the passage of the gas through the exit-tube
is checked by the increase of temperature, thereby producing
increased pressure. This observation suggested the following
experiments on the effect of heat on the passage of gases
through capillary tubes.

Fig. 1.
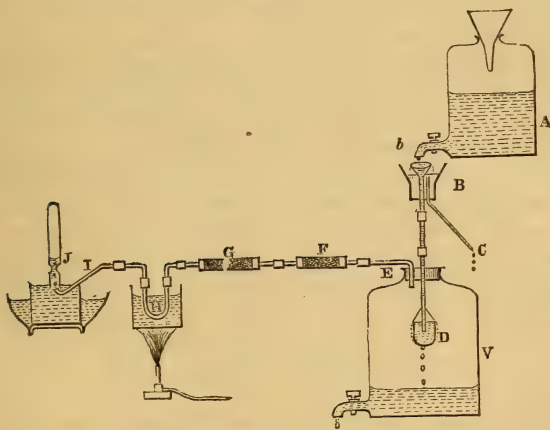
The apparatus used was as follows:—From a vessel, A,
water drops into a funnel, *b*, causing a continued overflow.
The overflow falls into the concentric funnel B, and escapes by
the tube C. The middle of the funnel-tube is removable, and
may be replaced by tubes of any required lengths. The lower

end of the funnel-tube passes air-tight through the cork of a bottle, V, and has hanging from its end a little thimble of glass, D. By this means, whatever amount of water has entered V, the pressure in V is that due to the difference of height of the water in b and D. The air from V is dried by two chloride-of-calcium tubes, F and G, and then passes through the experimental capillary tube H, which can be heated. Thence it passes down the tube I and is collected over water in the tube J, which has a marked stricture. The vessel in which J stands is always overflowing.

It was found that the utmost attention was required to keep the air perfectly dry. A joint of caoutchouc in water is found to be porous to water near its boiling-point, the water probably penetrating as a vapour. As the slightest intrusion of water would vitiate the experiment, paraffin was used in such cases.

The first point to be decided on was, whether heating a current affected its rate of motion independently of its friction. At first sight this seemed not to be impossible; the expansion of the air while its temperature is being raised might, it was thought, react on the air behind it and thus check its outflow. Experiments, however, showed that this was not the case, and that the effect of heat in checking the current is due solely to its influence on gaseous friction.

It was found that the amount of air passing down a given capillary tube varies approximately in the inverse ratio of the square of the absolute temperature, and directly as the difference of pressure at the two ends of the tube.

Neither of these relationships, however, is quite exact; the following formula more nearly expresses actual results. Calling t the time required to fill with air a vessel of given capacity, and T being the absolute temperature reckoned from $-273°$ C., and denoting by p_1 and p_2 the pressures at the ends of the tube,

$$t = \frac{\alpha T^2}{p_1 - p_2}\left(1 + \frac{\beta}{T(p_1 - p_2)^n}\right),$$

where α is a small fraction depending probably on p_1.

The facts that the time varies approximately as the square of the absolute temperature, and is not exactly in inverse proportion to the difference of pressure, are worthy of notice; and their theoretical investigation will throw some light on the molecular theory of gases. The fact that the temperature affects the time according to the square of the absolute temperature is consistent with known molecular laws. It has been shown that the viscosity of air and its consequent shearing friction is in proportion to T. And it is obvious from the

fact that the volume of a given mass of air is directly as its absolute temperature, that the time required for the passage of a given quantity of air through a given tube at a given pressure should vary as T^2.

More anomalous are the results that the velocity does not exactly follow the simple law that the rates of passage at a given temperature are in proportion to the difference of pressures at the ends, but that the time of passage of a given mass of air is shortened by increased pressure in a somewhat greater proportion than the increase of pressure. The deviation from the law of inverse proportion is only slight, and obviously belongs to a term having a small absolute factor. But an inspection of the results will show that its existence is unmistakable.

To examine the effect of simply raising the temperature, the following experiments were made :—

(1) The current was heated in a wide tube before reaching the capillary tube, being afterwards cooled again to the temperature of the outer air. *Result* :—the time of passage of a given mass of air was slightly increased.

(2) The current was heated after passing the capillary tube. *Result* :—a slight decrease in the time of passage of a given volume. The decrease was only such as might be accounted for from the fact that the air was not completely cooled again.

(3) The heating and cooling of the current in the capillary tube. *Result* :—an increase of time apparently proportional to the length of the tube heated.

The following are the experiments :—

(1) Whole current at atmospheric temperature 22° C.

$$(a) \quad t = 659''$$
$$(b) \quad t = 656'' \quad \text{Mean } 657''\cdot5.$$

The current was then heated to 100° C. in a wide tube before entering the same capillary tube.

$$(a) \quad t = 670''$$
$$(b) \quad t = 668'' \quad \text{Mean } 669''.$$

The slight increase of time here indicated was probably due to the fact that the air current had not altogether regained its normal temperature before reaching the capillary tube.

(2) Whole current at atmospheric temperature 21° C.

$$(a) \quad t = 157''$$
$$(b) \quad t = 156'' \quad \text{Mean } 156''\cdot5.$$

The current was then heated to 100° C. in a wide tube after passing through the capillary.

$$(a) \quad t = 153''$$
$$t = 155'' \quad \text{Mean } 154''\cdot0.$$

(3) Capillary tube partly immersed in a paraffin-bath at 200° C. The end cooled to 22° C.

$$t = 10055''.$$

More of the tube being heated,

(a) $t = 1098''$
(b) $t = 1094''$ Mean 1096''.

The effect of heating and cooling of the same current in different tubes in the same circuit was then examined. The same tubes were timed separately and in conjunction at the same and at different temperatures.

Tube.	Temperature.	Time.
1	24.5 C.	60''
2	,,	57
3	,,	58
1	100	89
2	,,	81
3	,,	82
1+2	24.5	117
,, ...	100	173
1 } +2+3 }	100 } 21 }	203
1+2 } 3 }	100 } 21 }	228

These results show that the resistance of the sum is very nearly the sum of the resistances, whether all the tubes are at the same temperature or some at one temperature and some at another.

In order to ascertain whether the rate of passage followed any special law at the beginning or end of the tube, the time of the passage of a given mass of air thrust through a given tube at a given pressure and temperature was ascertained; the tube was then divided into parts, and the time of the passage of the same bulk of air through each part was measured.

	20° C.	100° C.
Entire tube	372.5''	563''
Part 1	89.5	133.3
,, 2	117.5	180.0
,, 3	63	96.5
,, 4	101	152
,, 1+2	208.5	315.6
,, 3+4	165	246.6

From this Table it is seen that, within the limits of experimental error, the time taken for the passage of a given bulk of air through the entire tube is equal to the sum of the times taken for the passage of the same amount of air through each of its parts.

A similar experiment with a tube divided into a greater number of parts was then made. The original tube was about 600 millims. long.

$$(a) \quad t = 85''$$
$$(b) \quad t = 83'' \quad \text{Mean } 84''.$$

The tube was then divided without loss into 21 parts; these were connected by caoutchouc and covered with paraffin.

$$(a) \quad t = 86''$$
$$(b) \quad t = 85'' \quad \text{Mean } 85''\cdot5.$$

These experiments show that the terminations of the tubes exert no special influence on the passage of the current.

A conical or trumpet-shaped tube was then examined, the arrangement being according to fig. 2. The following were the results.

Fig. 2.
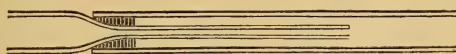
Wide end towards greatest pressure.

$$(a) \quad t = 183''$$
$$(b) \quad t = 184'' \quad \text{Mean } 183''\cdot5.$$

Narrow end towards greatest pressure.

$$t = 184''.$$

This shows that the rate is the same both ways.

The foregoing experiments having shown that the effect of temperature on the rate of a current was to be looked for in its influence on fluid-friction, the next thing was to determine the relation between temperature and time, other things being the same. For this purpose a capillary tube, through which a current was passing at a constant pressure, was subjected down its entire length to various temperatures—care being taken that the air should arrive at the entrance of the capillary tube at the temperature of the tube itself, so that the current might be of the same temperature throughout the entire length of the capillary tube. The times of passing of a given quantity of air for different temperatures were noted; and some of the results are subjoined.

			I. Pressure 315 millims. of Water.				
Temp. Centigrade. C.	Absolute temp. T.	T^2.	Time. t.	$\frac{T^2}{t}$.	$\Delta\left(\frac{T^2}{t}\right)$.	$\frac{T}{t}$.	$\frac{T(T+94)}{t}$
−20	253	64009	119″	538	12	2·42	712
0	273	74529	135·5	550	2	2·27	713
21	294	86436	156·5	552	15	2·13	705
60	333	110889	195·5	567	11	1·88	703
100	373	139129	240·5	578	36	1·65	696
200	473	223729	364·5	614	5	1·37	712
240	513	263169	425	619		1·29	713

			II. Pressure 1000 millims. of Water.				
20	293	85849	110	784·5	12·0	2·663	1035
40	313	97969	123	796·5	10·0	2·545	1036
60	333	110889	137·5	806·5	7·9	2·422	1034
80	353	124609	153	814·4	8·8	2·307	1031
100	373	139129	169	823·2	11·6	2·207	1031
120	393	154449	185	834·8	7·9	2·124	1035
140	413	170569	202·4	842·7	3·8	2·040	1034
160	433	187489	221·5	846·5	9·4	1·955	1030
180	453	205209	239·75	855·9	9·6	1·889	1034
200	473	223729	258·5	865·5		1·833	1035
							1033·5

These Tables show—

(1) That the time varies approximately as the square of the absolute temperature. This appears from the approximate equality of the numbers in column 5.

(2) That the variation of the time deviates from the law of squares by a term approximately proportional to the temperature. This is shown by the average equality of the differences in column 6.

(3) That the formula connecting the time and the temperature is very nearly

$$t = d\mathrm{T}(\mathrm{T} + \beta),$$

where β seems to depend on the pressure $p_1 - p_2$.

From the above series of experiments, however, the exact connexion between β and $p_1 - p_2$ is not obvious; possibly it may be of the form

$$t = \alpha\mathrm{T}\left(\mathrm{T} + \frac{\beta}{(p_1 + p_2)^3}\right).$$

The next series of experiments had in view the more exact determination of the relation between $p_1 - p_2$ and t.

To obtain these results, it was necessary to determine the

effect of capillarity and bubble-tension on the exit of the air through the final orifice.

It was found from experiment that these forces were very nearly equivalent to a pressure of 22 millims. The depth of the exit-orifice under the surface of the water over which the air was collected had, of course, to be allowed for.

The experiments gave the following results :—

<div align="center">Temperature 20° C.</div>

	$p_1 - p_2$.	t.	$(p_1 - p_2) \times t$.	$\Delta(p_1 - p_2)t$.
I.	millim. +15 76 157 303 513 656 852 1205	3945″ 719 344·5 180·5 105 81 61·75 43·6	59175 54644 54086 54691 53865 53136 52611 52538	−4531 − 558 + 605 − 826 − 729 − 518 − 73
II.	200 400 600 800 1000	426·5 209·5 139 . 103·6 82	85300 83800 83400 82933 82000	−1500 − 400 − 467 − 933
III.	200 400 600 800 1000 1200	563·6 278 . 184·6 137·3 108 89	112733 111200 110800 109866 108000 106800	−1533 − 400 − 934 −1866 −1200

The approach to equality in the amounts in column 3 shows that the time varies nearly in the inverse ratio of the difference of pressures. The gradual diminution of the number in that column visible, on the whole, shows that this law is slightly deviated from, and that the time diminishes somewhat more rapidly than the reciprocal of the pressure—a conclusion which is at variance with the results arrived at by Bunsen for the passage of gases through porous plugs.

LX. *On the Electric Conductivity and Electrolysis of Chemical Compounds.* By Dr. L. BLEEKRODE.

7. *Electrolysis of some simple Carbon Compounds.*

I HAVE here put together a few compounds in which the carbon present is in a simple relation to the other element. They are all liquid at ordinary temperatures, with the excep-

tion of cyanogen, which was easily condensed by heating dry cyanide of mercury in a closed tube. The results of the experiments are comprised in the following Table.

TABLE VI.

Compound.	Galvanic battery, 80 B. el.	Induction-coil.	
		Spark-length = 15 millims.	Spark-length = 70 millims.
Cyanogen[1], $C_2 N_2$	$D = \overset{\circ}{0}$	$D = \overset{\circ}{0}$ No spark-discharge.	Spark-discharge.
Carbon bisulphide[2], CS_2	$D = 0$	Spark-discharge.	
Carbon dichloride, $C_2 Cl_4$	$D = 0$	$D = 4$ No spark-discharge.	Spark-discharge.
Carbon trichloride[3], $C_2 Cl_6$	$D = 0$	$D = 2$ Heat evolved.	Spark-discharge.
Carbon tetrachloride, $C Cl_4$	$D = 0$	$D = 0$	Spark-discharge.

[1] The sparks were of blinding whiteness; carbon deposited.
[2] Distance between electrodes = 1 millim.; carbon deposited.
[3] Fusible at 160° C.

The experiments agree in showing that all these compounds are extraordinarily bad conductors of the galvanic current. I have been able to demonstrate this in the case of the first two with a much more powerful battery (§ 9).

In its chemical relation cyanogen, although quickly decomposed when dissolved in water or ammonia, as a condensed gas shows itself an extremely inert substance and a very feeble solvent. Gore* found that of 32 substances only 14 were dissolved by it, and none exhibited powerful chemical action.

Bisulphide of carbon, however, decomposes in contact with certain compounds, and with electrolytes exchanges constituents through elective affinity. It is dissolved by caustic potash; and then potassium carbonate and a sulphocarbonate are formed. With a solution of ammonia a sulphocarbonate and sulphocyanate of ammonium are produced. And yet it does not transmit the current of a battery of 950 Bunsen elements.

The carbon chlorides are also nearly insulating; sparks of 50 millims. length, from an induction-coil, will not pass through a distance of 10 millims. in tetrachloride of carbon,

* Proc. Roy. Soc. vol. xx. p. 67.

but glide along the inner surface of the tube. But these bodies are decomposed through elective affinity (by caustic potash for instance) only after a long time and with the aid of heat; and acids act on them only when a metal is present. This behaviour is in accordance with Hittorf's theory; and with respect to the part which hydrogen might play in electrolysis, it is to be remarked that these nearly insulating compounds are precisely those which contain no hydrogen *.

8. *Electrolysis of some Hydrocarbons and Haloid Compounds of the Alcohol-Radicals.*

Only two of these compounds which I have investigated (diethyl and dimethyl) required to be condensed. This was easily effected, as they were prepared by Frankland's method from iodide of ethyl and iodide of methyl by decomposition with zinc, heating them in closed tubes. The results are collected in Table VII. (pp. 442, 443). As some of these bodies are indifferent to acids and do not exchange their hydrogen for metals, while others exhibit reciprocal decomposition, they afford an opportunity for again testing the before-mentioned hypotheses. The hydrocarbons examined behave to the galvanic current as very bad conductors. Their chemical behaviour to the strongest acids, such as nitric and sulphuric, varies. Diethyl is quite indifferent, and undergoes no decomposition; and so is dimethyl to sulphuric acid; amylene is split up by sulphuric acid; and these substances form derivatives only by combining directly with the halogens. Benzol, toluol, and naphthaline are extremely bad conductors, and therefore would probably be also chemically neutral. But in these the hydrogen is readily replaced by the halogens†: they are directly decomposed by sulphuric or by nitric acid, water being separated and sulphacids or nitro-compounds produced; otherwise the alkalies do not attack them, or only at high temperatures. As, then, here neither is hydrogen exchanged for metals, nor does decomposition by proper elective affinity take place, these compounds support my hypothesis, as well as that of Hittorf; and this holds good for their substitutes also, as bromide of benzol (a bad conductor which is not decomposed by caustic potash or acetate of silver), and also for nitro-compounds.

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* The same remark holds good for the anhydrides consisting of oxygen and a metalloid (Table II.) and for NO_2.

† Phenate of potass ($C_6 H_5 OK$) is decomposed even by the current of 5 Bunsen elements, with brisk liberation of gas.

TABLE VII.

Compounds.		Galvanic battery.		Induction-coil.	
		20 B. e.	80 B. e.	Spark-length =15 millims.	Spark-length =70 millims.
Diethyl[1],	$(C_2H_5)_2$	D 0°	D 2°	D=2	D=10
Ethylic chloride,	C_2H_5Cl	2	3	....	Spark-discharge.
Ethylic iodide[2],	C_2H_5I	Trace.	2 (G=0)	D=5	Strong heating; iodine separated.
Ethylic sulphide[3],	$(C_2H_5)_2S$	Trace.	2	D=20	
Ethylic bromide[4],	C_2H_5Br	...	2	Brisk effervescence.	
Ethylene bichloride,	$C_2H_4Cl_2$	0	3 (G=0)	Spark-discharge.	
Ethylene bromide,	$C_2H_4Br_2$	0	0	....	No effect.
Mercaptan[5],	C_2H_5SH	0	...	Traces of conduction.	
Dimethyl[6],	$(CH_3)_2$	0	0	....	D=4 Brisk effervescence.
Methylic iodide[7],	CH_3I	4	...	Much heat evolved.	D=5 Spark-discharge.
Amylene[8],	C_5H_{10}	0	0	D=0 No effect.	Spark-discharge.
Amylene bromide,	$C_5H_{10}Br_2$	2	4	D=0	D=15 Spark-discharge.
Amylic chloride[9],	$C_5H_{11}Cl$	...	4	D=4 Spark-discharge.	D=30
Amylic bromide[10],	$C_5H_{11}Br$	0	4 (G=0)	D=0	D=6
Amylic iodide[11],	$C_5H_{11}I$	0	0	D=0	D=10 Spark-discharge.
Amylic sulphide[12],	$(C_5H_{11})_2S$	...	0	D=0 No sparks.	
Amylic alcohol[13],	$C_5H_{11}OH$	0	0	No effect.	

[1] A thick mist appears, and effervescence; no spark-discharge.
[2] Distance between electrodes=1 millim.
[3] Spark-discharge; liquid coloured yellow; spark green.
[4] Decomposition by induction-current; liquid becomes yellow.
[5] A single spark now and then passes.
[6] No spark-discharge.
[7] Liquid grows very hot through the discharges, but is not coloured.
[8] Sparks green-coloured.
[9] Discharge turns the liquid black.
[10] Effervescence; liquid turns yellow.
[11] Much iodine separated at the discharge.
[12] Electrodes 3 millims. apart; undulations of the liquid, and vibration of the longer electrode.
[13] Lapschin and Tichanowitsch found some conductivity with 900 B. e. and the electrodes 1 millim. apart.

Table (*continued*).

Compound.		Galvanic battery.		Induction-coil.	
		20 B.e.	80 B.e.	Spark-length =15 millims.	Spark-length =70 millims.
		D	D		
Allylic trichloride[14],	$C_3 H_5 Cl_3$	15°	...	Spark-discharge.	
Allylic tribromide[15],	$C_3 H_5 Br_3$	3	15°	D=0°	Spark-discharge.
Allylic iodide[16],	$C_3 H_5 I$	6	20	D=0	D=15°
				No heating.	Spark-discharge.
Allylic sulphide[17],	$(C_3 H_5)_2 S$	0	...	D=0	
				No effect.	
Benzol[18],	$C_6 H_6$	0	0	No effect.	
Brombenzol[19],	$C_6 H_5 Br$	0	0	No effect.	D=20°
					Spark-discharge.
Nitrobenzol[20],	$C_6 H_5 (NO_2)$	0	...	D=0	
				No effect.	
Binitrobenzol[21],	$C_6 H_4 (NO_2)_2$	6	...	D=20	
Phenol[22],	$C_6 H_5 O H$	10	...	D=30	
				Spark-discharge.	
Nitrophenol[23],	$C_6 H_4 (NO_2) O H$	2	...	D=0	
				No effect.	
Trinitrophenol[24],	$C_6 H_2 (NO_2)_3 OH$	2	...	D=10	
				Decomp. without spark-discharge.	
Toluol[25],	$C_6 H_5 CH_3$	0	0	Spark-discharge.	
Bromtoluol[26],	$C_6 H_4 Br CH_3$	0	0	Spark-discharge.	
Nitrotoluol, $C_6 H_4 (NO_2) CH_3$, { Ortho-[27]		2	...	Spark-discharge.	
Para-[28]		2	...	D=10	
				Spark-discharge.	
Binitrotoluol[29],	$C_6 H_3 (NO_2)_2 CH_3$	2	...	Spark-discharge.	
Naphthaline[30],	$C_{10} H_8$	0	0	No effect.	No effect.
Nitronaphthaline[31],	$C_{10} H_7 (NO_2)$	5	30	D=40	Spark-discharge.
				Spark discharge.	
Binitronaphthaline[32],	$C_{10} H_6 (NO_2)_2$	8	40 and de- comp.	....	Spark-discharge.
Nitroaniline[33],	$C_6 H_4 (NO_2) NH_2$	90	...	D=90	

[14] Electrodes 5 millims. apart; carbon separated by the sparks.
[15] Carbon separated by the discharge.
[16] Decomposition by the discharge; brown vapours evolved.
[17] Electrodes 1 millim. apart; vibration of longer electrode.
[18] In one direction the 15-millim. sparks strike through with a bright blue colour. Electrodes $2\frac{1}{2}$ millims. apart.; longer electrode agitated.   [19] The liquid turns black.
[20] Vibration of longer electrode; distance between electrodes 2 millims.
[21] Fusible; no spark discharge.   [22] Distance between electrodes 2 millims.
[23] Fusible at 45°; slight vibration of longer electrode; electrodes 3 millims. apart.
[24] Fusible at 42°; electrodes 4 millims. apart; gas evolved at the electrodes.
[25] Without spark-discharge a wave motion in the liquid.
[26] Single sparks appear; electrodes 1 millim. apart.
[27] Liquid; electrodes 3 millims. apart; liquid coloured brown by the discharge.
[28] Brisk effervescence with the spark-discharge; liquid turns black.
[29] Distance between electrodes 2 millims.; deflection of 8°, only with the discharge; feeble vibration of longer electrode.
[30] Fusible at 79°; electrodes 5 millims. apart; very bad conductor.   [31] Fusible at 43°.
[32] Fusible at 210°; distance between the electrodes 4 millims.
[33] Rapid decomposition by the induction-current.

Nitrobenzol is a bad conductor, and is only feebly attacked by the alkalies; and so is binitrotoluol, which, indeed, is scarcely at all acted on by fuming nitric acid even at a high temperature.   On the other hand, binitronaphthaline is a very good conductor which is also decomposed by the alkalies and by sulphides.

Conversely, the Table comprises substances in which accordance of this kind does not subsist.   Phenol is a bad conductor, although the hydrogen of its hydroxyl can be replaced by potassium or sodium *, and the hydrogen of the radical by the halogens or atomic groups like NO_2, NH_2, &c., mostly without the aid of heat.   It is true that double decomposition with the alkalies does not take place; phenol cannot, for instance, expel the carbonic acid from carbonate of potass. The behaviour of nitrophenol is precisely the same, which yet decomposes carbonate of magnesia.   Trinitrophenol possesses very powerful acid properties, and forms salts with soluble bases by reciprocal decomposition; metals take the place of its hydrogen; nevertheless it shows scarcely a trace of conduction with the current of 20 elements, and decomposition first takes place when the induction-current is employed.   On the contrary, nitroaniline is a good conductor, although it furnishes no combinations with metallic salts by double decomposition.

The haloid compounds of the alcohol-radicals often exhibit a certain degree of conductivity for powerful galvanic currents; for a galvanometer-deflection of from $2°$ to $4°$ is observed, which is much greater with the allyl compounds.   Their chemical character agrees with the hypotheses before-mentioned in so far, that they are decomposed by soluble bases, a metallic salt and an alcohol being formed, though for this it is frequently necessary to raise the temperature.   These substances are also directly acted on by some metals—for example, ethylic iodide by magnesium, at ordinary temperature †, diallyl being formed; the silver-salts, too, are decomposed by them.

Again, the sulphides of the alcohol-radicals do not behave in accordance with the hypotheses, since they appear to be bad conductors, and yet allylic sulphide, which does not give passage to the induction-current, with nitrate of silver forms sulphide of silver by double decomposition.   Still more striking in this respect is the behaviour of mercaptan, which exchanges

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* Phenate of potassium ($C_6 H_5 OK$) is decomposed, with brisk liberation of gas, by the current from 5 Bunsen elements.

† Gladstone and Tribe (Proc. Roy. Soc. vol. xxvi. p. 2) recently found no galvanometer-deflection, with ethylic iodide, for the current from 100 Grove's elements.

its hydrogen for potassium and sodium*, and, in alcoholic solution, also for copper, gold, &c.; indeed chloride of gold is violently attacked, hydrochloric acid being liberated, and still more violently the oxide of gold, at ordinary temperature. Amylic alcohol scarcely gives passage to the current of 950 Bunsen elements, although potassium and sodium expel its hydrogen.

The transmission of induction-currents through liquids is a very complicated phenomenon, in which it is often difficult to decide whether electrolysis has taken place, or whether merely the thermal action of the spark-discharge has split up the compound. The electrodes may be placed so far apart that the induction-current (in truth, only the opening-current) will be transmitted without any spark-discharge, and a sensitive galvanometer show a deflection (in methylic iodide, for instance, in which a great rise of temperature was at the same time perceptible); or with the electrodes at a less distance from one another, spark-discharges will take place (as in the allylic compounds, in which carbon will then be deposited), and during these the needle will be deflected. The peculiar conduction previously mentioned, in which a wave-like motion of the liquid accompanies the transmission of the induction-current†, makes its appearance also in amylic sulphide and in toluol: in brombenzol and allylic sulphide only the longer electrode is put into vibration; while in bromtoluol nothing of the kind was to be observed with the distance of 1 millim. between the electrodes, although this substance is a very bad conductor. In naphthaline a spark of 70 millims. will not burst through. Accordingly this kind of discharge appears to occur only at a certain degree of insulation.

On the whole, then, the chemical properties here give no measure for the conductivity; from them no conclusion can be drawn *à priori* respecting the latter.

9. *Experiments with Warren De la Rue's Chloride-of-silver Battery of* 8040 *elements and some very badly conducting Compounds* ‡.

Notwithstanding the foregoing experiments it still remains an open question whether on employing very much more powerful currents conduction and decomposition would not

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* Mercaptan with mercuric oxide produces ethyl-sulphide of mercury; this compound is readily fusible, and conducts the current of 20 Bunsen elements (galvanometer-deflection 3°).

† In Table III. with triethylamine, and Table V. with mercury-methyl, where the electrodes were still 4 millims. apart.

‡ See a brief account of these results in the Proceedings of the Royal Society, vol. xxv. p. 322 (1876).

take place in those substances which are regarded as insulating. It is certain that for every compound there is a temperature at which it is decomposed by heat ; and similarly it may be imagined that a current of sufficient intensity would produce electrolysis of any compound.

I therefore thought it of great importance for the purpose of this investigation to employ the current of the chloride-of-silver battery of Warren De la Rue, the most powerful that has ever been made.   Hence I feel bound thus publicly to return him thanks for the readiness with which he acceded to my request, and for his valuable aid in the experiments undertaken by us in common.

The battery * comprised, at the time of these experiments, 8040 cells, which could either be divided into various series or employed all together.   The greatest length of spark in air and between copper electrodes was 8·5 millims.   I had not, it is true, an opportunity of testing with this current all the previously investigated substances ; but those submitted to it are among the most important chemical compounds.

I. *Ammonia.*—On transmitting the current of 3240 elements the results previously obtained with the battery of 80 Bunsen elements were confirmed ; only, of course, the action was much more powerful.   Dark-blue currents were visible in the liquid ; and the positive electrode assumed a black colour ; at the same time there was abundant liberation of gas.   As soon as the current was interrupted the blue vanished and the liquid was clear again.

II. *Bisulphide of Carbon.*—We first applied the current of 3240 elements.   The negative electrode, some inches in length, was repelled from the surface of the liquid till it came into contact with the glass of the tube ; and, from some particles floating about in various directions, it appeared that internal motions took place, possibly caused by heat, though the hand did not detect any elevation of temperature.   The current from 5640 cells was too powerful : the sparks leaped between the electrodes, which became covered with a brown colour from the deposition of carbon.   We afterwards tried with a very sensitive Thomson's galvanometer to ascertain if there was any polarization-current ; but no such current could be detected.

III. *Benzol.*—With 3240 cells a strong vibratory motion was observed in the liquid.   With 5640 cells this motion was increased in force and became more apparent ; also a sound was heard very similar to that emitted by the contact-breaker

* See Pogg. *Ann.* vol. cxxxv. p. 496 (1868), and vol. clvii. p. 290 (1876).

of a moderate induction-coil. This proceeded from the longer electrode, which, extending the whole length of the tube, could move somewhat freely : the passage of the current set it in vibration ; and undulations were seen at the surface of the benzol. This phenomenon was most pronounced when the longer electrode was the negative pole ; the surface of the liquid was then depressed around the wire ; but when it was positive, the reverse was the case—the vibrations feebler, and the fluid ran up the wire. We found no indication of a polarization-current.

This peculiar phenomenon of the vibrations is analogous to a remarkable experiment previously discovered by Warren De la Rue : when one electrode of the battery of 8040 cells ends in a horizontal flat disk of copper and the other in a very thin platinum wire placed above it a little beyond the striking-distance, the electricity streaming out of the wire sets it in oscillation so that its extremity describes luminous circles, often ellipses. If the platinum wire is positive, the vibrating motion is feebler and consequently the luminous circles smaller.

IV. *Stannic tetrachloride.*—The current from 8040 cells transmitted through the liquid produced strong vibrations when the longer wire was the negative electrode ; when positive, they were weaker. We could not detect either a polarization-current or any trace of electrolysis.

V. *Carbonic acid* (CO_2).—We transmitted the current from 5640 cells directly through the gas condensed to the liquid state. With the small space between the electrodes this discharge was too strong, since a brisk succession of sparks appeared, the heat produced by which caused the tube to explode. Notwithstanding, the conclusion was thereby justified that liquid carbonic acid is a very bad conductor, since with this current a spark-discharge does not take place in conducting fluids.

VI. *Hydrochloric acid.*—The condensed acid was prepared in the closed tube from dry chloride of ammonium and concentrated sulphuric acid, by which the gas was deprived of moisture simultaneously with its evolution.

At first a series of 2160 cells was used, but without result, no action being perceptible. We then tried the current from 3240 cells, when the vibrations in the liquid were evidenced by the undulating surface. Finally 5640 cells were employed ; and now the peculiar humming sound from the vibrating electrode was also heard. Taught by the preceding experiment, we did not employ the entire battery, for fear of an explosion ; but without this the present experiment was

decisive for the conclusion that this gas (which when dissolved in water is one of the best conductors) opposes when liquefied by condensation a formidable resistance to an extraordinarily intense galvanic current, and is not sensibly decomposed by it.

VII. *Cyanogen.*—The current from 3240 cells produced no effect on the liquefied gas. With 5640 cells the vibrations were very apparent, and the same difference between the negative and the positive electrode was observed as in the case of benzol : the liquid ran up the wire when it was connected with the positive pole. No polarization could be detected.

VIII. *Zinc-ethyl.*—In this liquid vibrations were excited by 3240 cells. They were especially distinct when 5640 cells were employed ; and at the same time the temperature of the fluid was sensibly raised. Afterwards we detected a current caused by polarization of the electrodes, which produced a deflection of 5 divisions on the scale. We have consequently reason to conclude that electrolytic action took place, which may therefore be accompanied by vibratory motions, although for this a very powerful current is requisite.

IX. *Benzol.*—The previously mentioned experiment (III.) with benzol was repeated, in order to ascertain, if possible, whether a galvanic current could pass through a compound without electrolysis. The entire series of 8040 cells was now employed ; and the discharge was accompanied by sparks between the electrodes. A copious deposit of carbonaceous matter coloured the liquid ; so that another tube had to be prepared, in which the electrodes were separated by a greater distance. The current was maintained during two minutes : there was no spark ; but the liquid was in continual vibration, indicating the transmission of electricity. Afterwards, however, not the slightest polarization-current could be detected by the galvanometer ; and the electrodes had remained perfectly clean. It must nevertheless be remarked that, with extremely badly conducting liquids, the absence of a galvanometric indication of a polarization-current does not permit the conclusion that absolutely no electrolysis has taken place ; for the great resistance of the benzol, which had to overcome the current of the entire battery, so much diminished the intensity of the latter that it could only produce a deflection of about 55 divisions on the scale of the sensitive Thomson galvanometer ; and the feeble current that may have resulted from polarization, being opposed by the same resistance in the liquid, would therefore be unable to act on the galvanometer.

When the same current was transmitted through pure water,

a copious amount of gas was immediately evolved, but no vibratory motion was perceptible in the liquid : this (as already remarked) appears only to come into play with a certain degree of insulation and a determinate difference of potential between the electrodes. It is very possible that we have then to do with the discharges named by Faraday "carrying discharges," in which the greater portion of the electricity is mechanically conveyed by the molecules without their splitting up as in the case of electrolysis.

10. *Conclusions.*

The chief object of this investigation was to establish the possibility of electrolysis of a great variety of compounds, in order to determine the part which might be taken by hydrogen in electrolytic conductivity in consequence of its chemical character. Now it appears from the foregoing experiments that upon this definitive conclusions cannot be established ; and even elective affinity has no preponderating influence.

It is true that the hydracids, with the exception of hydro-cyanic acid, are very bad conductors, and their electrolysis is far from being proved ; their chemical properties in the liquefied state are very feeble ; but this may result from ex-traneous causes—for example, the very slight solvent power of the liquefied gas. The frequent coincidence therewith of the insulating property may therefore be regarded as to some extent accidental.

On the other hand, a certain number of organic compounds, as the organo-metallic radicals and some haloid compounds of the alcohol-radicals, readily exchange their metal or hydrogen for other metals (or groups of atoms, as, for instance, in the benzol series and most of the amines), and yet are bad con-ductors. It is, however, remarkable that no compound with-out hydrogen or a metal is known as a conductor, although the mere presence of one of these elements seems not sufficient to determine conductivity. The behaviour of the anhydrides particularly supports this opinion. Also there do not appear to be any substances that are good conductors without being readily decomposed by chemical reactions.

The view which is now more and more generally admitted in physics, that electric conductivity is not a simple function of the chemical composition, therefore receives fresh confirma-tion from my experiments. An analogous conclusion in regard to another class of compounds—namely, fused salts, consequently bodies that are also liquid without a solvent—has been established by Braun[*]. He concludes from his in-vestigation that no connexion is recognizable between galvanic

* Pogg. *Ann.* vol. cliv. p. 195.

conductivity and other physical or chemical properties (molecular weight, molecular volume, &c.).   The main proposition of Hittorf's theory of electrolysis, according to which the difficulty of exchange of the constituents is connected with the resistance of electrolytes, derives no support at all from my experiments.   Hittorf always tried the compounds in solution ; and for that very cause it must be hazardous to deduce therefrom definite conclusions respecting their conductivity.   It has recently been proved by F. Kohlrausch, Herwig, and others, that liquids which are bad conductors become more conductive through mixture with other substances, even when these are themselves bad conductors. Besides, we have in mercaptan, hyponitric acid, bisulphide of carbon, the organo-metallic radicals, &c. instances of compounds which easily exchange their constituents with salts, acids, or water, and yet behave to very powerful electromotive forces as almost insulating substances.

A newer conception, based by G. Wiedemann, and after him by F. Kohlrausch, upon their experiments, takes chiefly into consideration the mechanical processes going on in electrolysis, such as friction of the molecules against one another, conveyance of the ions with different velocities, &c., points which in other theories remain quite in the background.   The great resistance of liquids is then explained by the frequent collisions between the constituents.   The solvent gives opportunity for the molecules to move through comparatively great distances before meeting one another and again combining. It is therewith mentioned that not a single liquid is known which at ordinary temperature is by itself a good conducting electrolyte [*].   My experiments, however, point out liquid ammonia and hydrocyanic acid as substances which are clearly decomposed even by moderate galvanic currents.   Although there may be reasons for these assumptions [†], and the rest of the experiments point to the conclusion that liquids in general are very bad conductors, while by the above assumption the remarkable phenomenon of the diminution of the resistance by mixing two compounds both of which are bad conductors, yet I cannot reconcile therewith the fact that a salt when fused is notwithstanding a good conductor—for instance, chloride of silver.   Here the melting-point is far below the temperature

[*] Pogg. *Ann.* 1876, p. 253.
[†] F. Kohlrausch, before he was informed of my experiments, expressed the anticipation that liquid hydrochloric, hydrobromic, and hydriodic acids would prove insulating (*l. c.* p. 278) ; I have found this fully confirmed by experiment.   On the other hand, he reckoned hydrocyanic acid and ammonia in solution among the worst conductors ; in the condensed state they proved, in my experiments, to be very good conductors and electrolytes.   This exceptional behaviour is very remarkable.

at which decomposition takes place ; so that there is no valid reason for ascribing the increased conductivity to the weakening of chemical affinity by heat ; and yet the molecules must here encounter one another at least as frequently as in a compound liquid at ordinary temperatures.

It is also difficult, if solution is a necessary condition for electrical conductivity, to imagine compounds already electrolyzed in the solid condition, such as the haloid compounds of lead [*], oxide of zinc, sulphide of antimony, and realgar [†], in the electrolysis of which the zinc and sulphur separated ignite in the air.

The opinion has been expressed, that, on applying extremely intense electromotive forces, even those compounds which are insulating to the current of an ordinary galvanic battery would assume an electrolytic conduction. But the highly intense current from Warren De la Rue's chloride-of-silver battery does not effect any evident electrolysis of the compounds which had shown themselves the most insulating, but leaves it doubtful. Still, from the undulatory motion of the liquid, a real transmission of electricity must be inferred. I have succeeded in bringing about the same phenomena by means of moderate currents from a Ruhmkorff's induction-coil, as well as by the discharge-current of a powerful electrical machine, in liquids possessing a certain degree of insulating-power, and with a determinate distance between the electrodes. This peculiar kind of propagation of electricity is consequently to be observed with simpler apparatus than I at first believed [‡]. More experiments on this subject I hope to communicate hereafter ; I will now only remark that a fresh argument is thereby furnished against the existence of a so-called metallic conduction in compound substances, since extremely intense galvanic currents are transmitted through badly conducting liquids in the same way as induced currents; and we may regard this transmission as effected by rapid successive self-discharges of a condenser, in which the liquid layer between the electrodes plays the part of insulator, and the electrodes form the coatings [§].

The Hague, March 1878.

[*] E. Wiedemann, Pogg. *Ann.* vol. cliv. p. 319.

[†] Lapschin and Tichanowitsch, *loc. cit.*

[‡] Lapschin and Tichanowitsch (*op. cit.*) state that in ether an undulatory motion was produced from the positive to the negative pole by the current from only 900 Bunsen elements.

[§] Herwig (Pogg. *Ann.* vol. clix. p. 83) arrived at the same representation by a quite different train of reasoning. I take this opportunity to point out to chemists how desirable it is that they should investigate the electrolytic properties of the new substances discovered by them, as generally these are only with difficulty accessible to physicists.

LXI. *Theoretical Explanations of the Actions of the Radiometer, the Otheoscope, and the Telephone.* By Professor CHALLIS, *M.A., F.R.S., F.R.A.S.*[*]

I HAVE included the proposed explanations of the actions of the three instruments in the same communication, because in each case the theoretical reasoning rests on those hypotheses respecting the qualities of the æther and of atoms which I have so often defined in this Journal, and, again, because the explanations viewed in relation to each other serve to confirm the truth of the hypotheses. The constituent atoms of any substance are conceived to be inert, spherical, and of constant form and magnitude ; and the æther is considered to be capable of producing cognizable phenomena only so far as it may be mathematically treated as a simple continuous fluid of unlimited dimensions, susceptible of variation of density, and pressing always and everywhere in exact proportion to its density. The supposed variation of density implies that the æther is atomically constituted; but in the physical theories I have proposed this constitution does not specifically come under consideration. It may be strictly proved on hydrodynamical principles that the motions of such a fluid cannot permanently alter the quantity of fluid matter on the opposite sides of any fixed plane of unlimited extent, and consequently that the motions are either vibratory, or steady circulating currents. These conditions being understood, the determination of the dynamical effect of the æther in motion on visible and tangible substances depends eventually on the solutions of two very definite hydrodynamical problems—namely, to find the motion of an atom (1) when it is acted upon by a steady current of the æther, (2) when acted upon by a series of harmonic æthereal vibrations.

The first problem is easy of solution ; and the general result, when powers of the velocity of the fluid above the second are omitted, is, that the acceleration of the atom has a constant ratio to that of the contiguous surrounding fluid (see ' Principles of Physics,' solution of example viii. p. 313, and the Hydrodynamical Theory of Attractive and Repulsive Forces, in the Philosophical Magazine for September 1872, art. 33). The solution of the second problem, when only the first power of the velocity of the vibrating fluid is taken into account, conducts, as might be anticipated, to motion of vibration of the atom, without permanent motion of translation. But when the square of the velocity is included, the pressure by which the motion of the atom is produced contains in its expression a term which has

for a factor the square of the harmonic function $m \sin(\alpha t + c)$, or $\dfrac{m^2}{2} - \dfrac{m^2}{2} \cos 2(\alpha t + c)$. The pressure on the atom is consequently partly such as a current corresponding to the indication of the first term would produce, and partly such vibratory motion as would be due to the pressure indicated by the second term. The former gives rise to motion of translation of the atom, and to acceleration of such motion. The acceleration may either be *from* or *towards* the source of the vibrations, according to the relation between the magnitude of the atom, and the breadth and intensity of the waves. The waves which emanate from individual atoms by reason of reaction from their surfaces, are all of such small magnitude that they act in the directions from the centres of the atoms on all surrounding atoms. This force is *atomic repulsion.* The composite waves resulting from the coexistence of those emanating from a vast number of atoms constituting a *molecule* may be supposed of such magnitude and breadth as to be capable of producing acceleration of exterior atoms in the direction *towards* the centre of the molecule. This is *molecular attraction.* The equilibration of the two kinds of forces maintains the aggregation of atoms which constitutes visible and tangible bodies. The arguments on which these statements are based are given in the article in the Philosophical Magazine for September 1872, already cited, and in a " Supplementary Discussion of the Hydrodynamical Theory of Attractive and Repulsive Forces," contained in the Number for September 1876. The statements are introduced here for the purpose of making intelligible the following General Theorems, to which the investigations referred to conduct:—

I. Assuming that the repulsive and attractive forces of nature are all resultants of modes of pressure of the æther, the actions of such forces on atoms are in every instance attributable to *æthereal currents,* whether the atoms be *immediately* acted upon by steady motions of the æther or by æthereal vibrations.

II. In any instance in which *a gradation of atomic density* exists in the interior of any body, in so far as this condition is persistent, molecular attraction in the direction from rarer to denser parts must be counteracted by atomic repulsion in the contrary direction ; and according to theorem I. these actions are accompanied by æthereal currents.

In the case in which an atom *in the interior* of a homogeneous body is kept at rest by atomic repulsion and molecular attraction, the resultant of each kind of force is zero, and there is consequently no direct evidence of the action or existence of æthereal currents. But if the atom be near the superficies

of the body, in order that it may be in equilibrium there must be (as Poisson first indicated) a diminution of density through a small superficial stratum in the direction towards the surface. Within this stratum the resultant atomic repulsion from within to without is controlled by molecular attraction from without to within. The latter force is due to æthereal currents extending beyond the boundary of the substance, being, in fact, the force which gives rise to the phenomena of capillary attraction.

But when the state of the atoms in the superficial stratum is violently disturbed, as by friction, the positions of equilibrium of all the atoms, both superficial and interior, are in some degree changed ; and in order that they may take new positions of equilibrium it is necessary that there should be a gradation of atomic density, and that the resultant atomic repulsion on any atom in the direction of decreasing density should be counteracted by the resultant molecular attraction on the same in the contrary direction. According to the particular constitution of the substance, the new state of equilibrium of the atoms is more or less persistent, and the substance is more or less *electric*. Now, by the above-stated general theorems, such actions, whether repulsive or attractive, are accompanied by æthereal currents which in their aggregate extend beyond the boundaries of the substance (much further than those above mentioned to which capillary attraction was attributed), and operate, in fact, in producing the mutual attractions or repulsions which are observed to take place between electrified bodies. These views are treated of in detail in ' The Principles of Physics,' pp. 505–555, and in the Philosophical Magazine for October 1860. I have referred to them here for the purpose of stating that the Theoretical Explanation of the action of the Radiometer which I have given in the Numbers of the Philosophical Magazine for May and November 1876, rests on the same principles—with, however, the difference that the disturbance of the superficial stratum, instead of being produced by friction, is considered to be due to the difference of action of light, or radiant heat, on the blackened and bright surfaces of the vanes. It is proved that, under these circumstances, æthereal currents are generated, the action of which on the component atoms of the vanes is such as to produce rotation in the direction in which the vanes of a freely suspended radiometer are observed to move. For these reasons I regard the motion of the radiometer as due to a force which is of the same kind as that which is induced in a body electrified by friction.

The explanation of the action of the *Otheoscope* is a simple corollary from the above theory of the action of the radiometer.

It is evident that, under the conditions of " otheoscope, No. 1," described in p. 177 of No. 180 of the ' Proceedings of the Royal Society,' the action of the light of a candle on the fixed plate blacked on one side is just the same as that on a vane, blacked on one side, of the revolving radiometer ; but as motion of the plate is prevented, there is a reflux of the æthereal current—the effect of which, judging theoretically, I should expect to be like that of wind emanating from the black surface, and (in accordance with Mr. Crookes's description) " blowing away whatever easily movable body happens to be in front of it, irrespective of colour, shape, or material," causing thus the rotation of the contiguous four-armed fly. In this respect the action resembles that of a body electrified by friction on small particles of any kind in its neighbourhood, which, in fact, under particular circumstances has been named *electric wind*.

Experiment has shown that a radiometer will rotate under the influence of light when both sides of each vane are equally bright, if only the vane be *cup-shaped*—that is, convex on one side and concave on the other.   This is a most remarkable fact, and may be regarded as an *experimentum crucis*, by which the truth of any proposed theory of the rotation of the radiometer may be tested.   As far as I am aware, no other attempt has been made to account for this fact than that which I have produced in the Number of the Philosophical Magazine for April 1877, supplemented by some remarks in a letter addressed to the Editors in the Number for May 1877.   The explanations there offered depend on the same principles of the hydrodynamical theory of physical force as those applied in the explanations of the motions of the radiometer and the otheoscope, with the addition of considerations respecting the variation of effect of the incident light at different points of the surface of the radiometer as depending on the convexity or concavity.   The theoretical result is, that the radiometer rotates as if pushed on the convex side, which agrees with experiment.

As respects the *Telephone*, an essential part of the explanation of its action is furnished by the general Theorems before enunciated.   In the ordinary use of this instrument the voice of the speaker, by setting an iron plate in vibration, is supposed to generate electric or galvanic currents, which being intensified as to their efficiency, and conducted, by known experimental methods, are made to act on another plate at a distant position, causing it to vibrate in such manner as to produce by agitating the contiguous air, sounds that correspond with more or less exactness to the pronounced words of the

speaker. These facts have received experimental explanations on the *hypothesis* of the generation of electric currents ; but *how* or *why* the means employed should produce such currents has not been accounted for. For the following reasons I consider that the hydrodynamical theory of physical force furnishes the required explanation. The component atoms of the plate which the speaker's voice puts in vibration, move to and fro by forces which result from the simultaneous action of atomic repulsion and molecular attraction, varying in direction according as the one or the other is in excess. Hence, as these forces are due to harmonic vibrations of the æther, according to the general Theorem I. æthereal *currents* are continually being generated, and coexist with the vibrations of the plate. In order that cognizable effects of these currents may be produced, it is necessary to provide means of *circulation* by conducting wires, or by *earth-connexion*, inasmuch as *all* æthereal currents, as already intimated, must satisfy this condition. The usual arrangements in the application of the Telephone do, in fact, fulfil this requirement. This theory of the action of the telephone is especially confirmatory of the *electrical* character of the æthereal streams to which in the previous explanation of the action of the radiometer the rotation has been ascribed.

An article " On certain movements of the Radiometer," dated November 1, 1877, is contained in No. 184 of the Proceedings of the Royal Society, p. 546. The author, Professor Stokes, has said nothing about " theoretical explanations " of the phenomena in the sense in which I use these terms in the title to the present communication ; but by " coordinating " the experiments he has arrived at conclusions of essential importance as regards the part which the theorist has to perform. He concludes that we must recognize, as distinct conditions under which motion may be obtained in a radiometer, (1) difference of temperature of the two faces, (2) curved form of the disk, (3) roughness of surface on one face ; but he does not decide whether condition (3) is different from (2). From a theoretical point of view, I should say that it is only required to take account of the effect of difference of temperature when there is no curvature, or the faces are plane, and to take account of the effect of curvature of the faces when there is no difference of temperature. The theories I have proposed account for the movements of the radiometer under the two conditions considered separately ; and hence, on the allowable supposition that these conditions operating conjointly produce independent effects, it may be said that all phenomena of the radiometer are thus accounted for on the principles of the general hydrodynamical theory of physical force.

In order to indicate the extent of the applicability of Theorems I. and II., I take occasion to add that whereas in the phenomena of frictional electricity the condition of gradation of atomic density is produced by disturbance of the superficial atoms of the electrified body, in the case of the phenomena of galvanic force the same condition is a necessary accompaniment of the *chemical* action going on in the battery; and in the case of the magnet a gradation of interior atomic density is maintained by the mutual counteraction of atomic repulsion and molecular attraction, and with more or less persistence according to the quality of the magnet and the degree of magnetization. In all three classes of phenomena æthereal currents are produced by reason of the gradation of atomic density, and as these currents are subject to the condition of *circulating,* it follows, as a consequence of such motion, that *polarity* is recognizable in electric, galvanic, and magnetic phenomena. This theoretical inference is confirmed by experiment.

Cambridge, May 16, 1878.

LXII. *On some remarkable Instances of* Crookes's *Layers, or Compressed Strata of Polarized Gas, at ordinary Atmospheric Tensions. By* GEORGE JOHNSTONE STONEY, *M.A., F.R.S.*[*]

1. IN a recent communication I gave some instances [†] of Crookes's layers at ordinary atmospheric tensions, and among them described one which accounts for the great

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[*] Communicated by the Author, having been read before the Royal Dublin Society, November 19, 1877.

[†] Viz. those which support light powders in heated capsules, or liquids in the "spheroidal" state, including the case of drops upon the surface of a liquid (see Phil. Mag. for last December, p. 441 *et seq.*). The theory of unequal stresses in polarized gas has thus fulfilled an anticipation which Mr. Crookes entertained so long ago as 1873, that whatever theory would account for the motion of radiometers would probably also explain the spheroidal state of liquids, and the mobility of finely divided precipitates in heated capsules; for he enumerates these among phenomena probably due, at least in part, to the same "repulsive action of radiation" as is manifested in radiometers. (See Philosophical Transactions, vol. clxiv. p. 526.) I was not aware of this passage when writing the paper on Penetration above quoted; otherwise I should have there referred to it. The heat which diffuses across a layer of gas passes under what are called "the laws of conduction" if the number of gaseous molecules present is sufficiently large. If fewer molecules are present, the heat passes under other laws, which may be distinguished from the laws of conduction by calling them the laws of penetration. In writing the paper on Penetration, I was under the misapprehension that the flow of heat by penetration between a heater and cooler of which the temperatures and

mobility that may be imparted to a light powder by heating it in a metal capsule. It is shown that in this case the powder floats on a stratum of air which it compresses by its weight, at the same time that it maintains the requisite polarized condition of the layer by radiating away its own heat so freely as to keep itself cooler than the capsule.

2. In exactly the same way we may explain a very curious phenomenon which has been recorded by travellers in Arabia, and to which Professor Barrett has directed my attention. There is in Arabia a mountain called Jebel Nagus, or Gong Mountain, which produces sounds resembling the booming of the Nagus, or wooden gong, used in Eastern churches instead of bells. The mountain consists of a white friable sandstone, which produces to the south-westward a great slope of very fine drift sand, and another smaller one to the north. The large one is 115 metres high, 70 metres wide at the base, and tapers towards the top. It is so steep, being inclined to the horizon at an angle of nearly 30°, and consists of such fine sand, that its surface can be easily set in motion by scraping away a portion from its base, or by disturbing it elsewhere. If this is done after the surface has been for a long time exposed to the sun, "the sand rolls down with a sluggish viscous motion, and the sound begins—at first a low vibrating moan, but gradually swelling out into a roar like thunder, and as gradually dying away" (Palmer's 'Desert of the Exodus,' vol. i. p. 218).

That heat contributes largely to the effect was proved by the valuable observations made by Captain Palmer; for it was found "that the heated surface was much more sensitive to sound than the cooler layers beneath, and that those parts of the slope which had lain long undisturbed produced a much louder and more lasting sound than those which had recently been set in motion."

Moreover, when the experiments were repeated on the other talus, which faced towards the north, and part of which was in perpetual shade, it was found "that the sand on the cool shaded portion, at a temperature of 17° C., produced but a very faint sound when set in motion, while that on the more exposed parts, at a temperature of 40°, gave forth a loud and even startling noise."

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positions are fixed, is greater than the flow of heat by conduction; whereas experiment shows that it is always feebler. Accordingly penetration is not be sought, as I supposed, in the whole of the shaded portions of the diagrams accompanying that paper, but only where the outline of them slopes downwards, *i. e.* between O and *n*. But this error does not affect the mechanical part of the paper, of which the present paper is a continuation.

These observations were made in winter. They clearly indicate that heat renders the surface of the slope more mobile by polarizing the air between the hotter and cooler particles of the sand.

The more intense the sunshine, the more powerful must the Crookes's layers be, and the more widespread will be the effect of any accidental disturbance. And if under the fierce glare of the tropical sun the strength of the Crookes's stress becomes sufficient to lift the uppermost grains of sand, the sliding motion, with its humming, booming, and thundering noise, will spring up without visible cause—a phenomenon that sometimes occurs, and has naturally occasioned much speculation.

3. Mr. Howard Grubb has directed my attention to another natural phenomenon which admits of being explained by the mechanical properties of polarized gas. In certain states of the weather *large* grains of sand, flat pieces of shell, and even flakes of stone of quite a considerable size may be seen floating on the tide as it flows in. I saw this phenomenon myself when a boy, but, unfortunately, did not make a careful examination of the attendant circumstances. It is, however, easy to see the conditions which would be most favourable to its production. They are :—a very powerful sun, to heat the stones and to maintain their temperature sufficiently high after they are set floating ; calm air, that no breeze may cool them ; a cold sea, to increase as much as possible the difference in temperature between the flakes of stone and the water ; and the absence of waves, that the heavy little barges may escape shipwreck.

I think it fortunate that I had written out the foregoing statement of the conditions indicated by the theory before I saw the following record of observations upon this phenomenon made by Professor Hennessy (see ' Proceedings of the Royal Irish Academy,' vol. i. series 2):—

" On the 26th of July, 1868, when approaching the strand at the river below the village of Newport, county Mayo, I noticed what appeared to be extensive streaks of scum floating on the surface of the water. . . . . . until I stood on the edge of the strand ; and I then perceived that what was apparently scum seen from a distance, consisted of innumerable particles of sand, flat flakes of broken shells, and the other small *débris* which formed the surface of the gently sloping shore of the river. The sand varied from the smallest size visible to the eye, up to little pebbles nearly as broad and a little thicker than a fourpenny piece. Hundreds of such little pebbles were afloat around me. The air during the whole morning was perfectly calm, and the sky cloudless ; so that although it was

only half-past nine, the sun had been shining brightly on the exposed beach. The upper surface of each of the little pebbles was perfectly dry; and the groups which they formed were slightly depressed in curved hollows of the liquid. The tide was rapidly rising; and, owing to the narrowness of the channel at the point where I made my observations, the sheets of floating sand were swiftly drifting further up the river into brackish and fresh water. On closely watching the rising tide at the edge of the strand, I noticed that the particles of sand, shells, and small flat pebbles, which had become perfectly dry and sensibly warm under the rays of the sun, were gently uplifted by the calm, steadily rising water, and then floated as readily as chips or straws."

The calm air, tranquil water, hot sun, and warm stones predicted from the theory are all recorded in these observations.

This rare phenomenon must not be confounded with the familiar one in which patches of *fine* sand float upon water in consequence of its surface-tension. The surface-tension of water in contact with air will not support flakes of stone of above a certain size; and those described by Professor Hennessy are at or beyond the limit of size* that could, even if separate, be floated by surface-tension. Hence they could not be supported by that agency *in the groups* which he describes. We are therefore forced to look elsewhere for the cause of the support of these groups: the thermal and mechanical properties of Crookes's layers show that they will suffice; and we have seen that all the conditions were present which would call Crookes's layers into existence.

4. Mr. George F. Fitzgerald has pointed out another very striking example. A piece of cold iron may be made to float on melted cast iron, and will even float high like cork on water. Here the difference between the temperature of the glowing mass of molten metal and the cold piece of iron is so considerable, that the stresses that are developed are able to support the weight of the piece of iron while it is still at such a distance from the fiery liquid that it seems to float high upon it. What it floats on is in reality a bath of polarized air, the stresses within which both support its weight and force down the surface of the molten metal. This air-bath keeps it out of

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* Taking the surface-tension of water in contact with air at 8·25 grammes per metre as determined at 20° C. by M. Quincke, and assuming 2·5 as the specific gravity of the stone, it follows that a circular disk, 16 millims. in diameter and 0·85 of a millim. in thickness, would be the extreme theoretic limit that could be supported by surface-tension. This is about the size of a fourpenny bit.

contact with the glowing mass; and accordingly it receives heat from below only by diffusion and radiation, in quantities far short of what it would receive from actual contact; and as it loses much heat by radiation upwards, it may be able for a considerable time to maintain a sufficiently low temperature to continue floating.

5. On the same principles we are to explain the safety of exploits that are occasionally performed; viz. the licking of a white-hot poker, the dipping of the fingers into molten metal, and the plunging of the hand into boiling water. In all these cases the Crookes's layers that intervene prevent that contact which would cause a dangerous scald or burn.

It is usual before performing these two latter experiments to moisten the hand with soapy water, ether, turpentine, or liquid ammonia. All of these would have the useful effect of lowering the surface-tension of the hot liquid, and thus diminishing the extent to which it would compress the Crookes's layer.

6. But the most splendid example I have yet seen of a Crookes's layer is one which was first noticed by M. Boutigny, and which was shown by Professor Barrett at the Brighton Meeting of the British Association, with the improvement of adding soap to the water—an addition which seems essential to the full success of the experiment. A copper ball, some 6 centims. in diameter, furnished with a staple by which it can be lifted, was brought to a bright red heat, and while glowing was lowered into a large beaker of soapy water. As the ball approaches the cold surface of the water, heat passes from the ball to the water by diffusion as well as by radiation; accordingly the intervening air becomes intensely polarized, and the Crookes's stress that accompanies the polarization makes a hollow in the surface of the water. Let the ball be lowered till it is half submerged; the depression in the water is now nearly hemispherical, but not quite so, since the interposed layer of polarized gas will be thinnest at the bottom, where, to withstand the pressure of the water, it must exert most force. The stresses at any point of this polarized layer consist of a constant stress P nearly equal to the tension of the open atmosphere, acting equally in all directions, along with a variable Crookes's stress p, acting for the most part nearly in the direction of a radius of the ball—the most marked deviation from this direction being close to the horizontal surface of the water, where the action of the upper hemisphere of the ball gives an inclined direction to the Crookes's stresses, and helps to round off the surface of the water. The amount of the Crookes's pressure acting on the water will vary with the depth, being

such at each point that it gives a component equal and opposite to the resultant of the pressure of the water at that depth, and of the surface-tensions round the point. Whenever the Crookes's force is not quite in the direction of this resultant, there will be a free tangential component; and this must produce surface-currents in the water. These, however, cannot be observed in the present experiment, because they are of small amount, and too much mixed up with convection-currents arising from the heat that reaches the water by radiation and diffusion.

When the ball is lowered until it is quite submerged, it will be surrounded on all sides by an envelope of polarized air, thinnest at the bottom (where the pressure of the water is greatest), thickest above. So long as there is any communication between the polarized layer and the atmosphere, the lateral stresses within the layer will be equal to P, while those in the direction in which the heat penetrates will be $P + p$; but both of these will suffer an increase if the ball is plunged deeper after the communication with the atmosphere has been cut off. No one can see this splendid experiment for the first time without a feeling of astonishment.

7. A Crookes's layer formed in the same way, but without the exquisite beauty which it has in this experiment, may be seen any day in a smith's forge, whenever the smith has occasion to quench white-hot iron in water.

8. A phenomenon closely resembling the experiment with the glowing ball was witnessed lately by my brother and two other friends while out walking. There was a shower when they reached some rather deep water. The afternoon had become chilly; and the phenomenon that presented itself shows that the water must have retained a temperature higher than that of the air. As the rain-drops fell into the water, some of them (estimated at one in twenty) became spheroidal drops floating on the water; and of these some (estimated at one in six) were visibly submerged before floating about as spheroidal drops. They sank, perhaps about half a centimetre, before they rose to the surface, and while under water looked like silvered pills, owing to the total reflection from the boundary between the water and the film of polarized air which enveloped each drop.

9. Several times, in the course of this communication, I have had occasion to speak of the feebleness of conduction or penetration, compared with the rapid outpour of heat which takes place on direct contact between a very hot and a cold body. This is well illustrated by an experiment of M. Boutigny, in which a spheroidal drop of water is formed inside a hot copper

bottle, and the neck of the bottle partially stopped by a cork through which a thin tube passes. So long as the drop continues in the spheroidal state, a mixture of air and vapour slowly escapes through the tube in the cork; but the instant the spheroidal state ceases, and the water comes into contact with the copper, a sufficient portion of the water flashes off so suddenly into steam that the cork is driven out with explosive violence.

10. A still more instructive illustration of these facts is afforded by the familiar experiment, known to every smith, that an explosion will occur if a little water is dropped on an anvil, if a white-hot strap of iron is laid over the drop, and if the iron is then given a tap with the sledge-hammer. In this experiment the hot iron, when laid on the anvil, does not fit it accurately, but comes into contact only at a few points, and leaves a chink elsewhere. While the iron is descending towards the drop of water, a Crookes's layer of polarized air is formed between it and the cold water, which exerts a sufficient pressure upon the drop, both to flatten it out, and to keep it from coming into contact with the glowing iron. At this stage of the experiment the lower portion of the chink is occupied by water, and the upper portion by polarized air. The stratum of air moderates the flow of heat towards the water, so that the water is able to continue liquid by parting with as much heat downwards to the cold anvil as it receives from above, before it is itself warmed beyond the boiling-point. But when the sledge-hammer descends, the soft iron yields, the chink is obliterated by a force greater than that which the Crookes's layer can support, and the glowing mass comes, in many places, into direct contact with the water. The vastly augmented flow of heat which is consequent upon this direct contact, rushes across the film of water with a speed equal to the velocity of sound in water, which will carry it across a film the seventh of a millimetre in thickness in the ten-millionth of a second. Within this brief period of time the greater part of the water is raised to a very high temperature; and its sudden conversion into red-hot steam causes the explosion.

Before concluding this communication I wish to take the opportunity of thanking my scientific friends for their kindness in bringing such remarkable instances of Crookes's layers at ordinary atmospheric tensions to my notice, and giving me permission to publish an account of them.

LXIII. *Water-Analysis. Determination of Cellulose and mo-
dified Cellulose in Drinking-Water.* By J. A. Wanklyn
and W. J. Cooper*.

THE ammonia process of water-analysis provides an index
to the nitrogenous organic matter in drinking-water; but
it does not deal with the non-nitrogenous organic matter, and
some method whereby the non-nitrogenous organic matter
might be reached is still a desideratum. This desideratum the
Frankland-and-Armstrong combustion process aims at sup-
plying, but (as is now pretty well understood among chemists)
fails to supply. The old permanganate process, which we owe
to Forchhammer, which notoriously failed in practice, would
accomplish the object in view if the practical difficulties which
beset it could be overcome.

If the quantity of oxygen required to oxidize cellulose or
sugar be calculated, it will be seen that rather more than its
own weight of oxygen is consumed by a given weight of cellu-
lose or sugar. The estimation, therefore, of cellulose or sugar
by means of the oxygen consumed in burning down to car-
bonic acid and water would rank along with the more advan-
tageous operations of analytical chemistry.

Forchhammer's process, as hitherto practised, does not effect
any thing approximating to complete oxidation down to car-
bonic acid and water, as was illustrated by some experiments
published by Frankland and Armstrong in 1868 (*vide* Chem.
Soc. Journ. vol. vi. p. 82), which I quote:—

Name of substance (30 parts dissolved in 1,000,000 parts of water).	Oxygen absorbed during six hours.	Oxygen required for complete oxidation.
Gum arabic . . .	0·35	35·5
Cane-sugar	0·15	33·7
Starch	0·30	33·5

showing that, as usually carried out, the oxidizing process
does not avail to accomplish more than about one hundredth
part of the task set before it. The manner in which the per-
manganate of potash is applied to drinking-water in Forchham-
mer's process is by simply mixing measured volumes of the
permanganate-solution with a known volume of the water to
be operated upon, and observing how many volumes are de-
colorized by the action of the organic matter. A quantity of

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* Communicated by the Authors.

acid is usually added to the water, and some length of time allowed to elapse before making the final readings.

The modifications whereby we have completely altered the character of the Forchhammer process are as follows :—

Instead of simply mixing the standard solution of permanganate with the water to be examined, we *distil* a given volume of the water (say, 1 litre) with a considerable excess of standard solution of permanganate, and thereby get more oxidizing action than in the ordinary operation. We find advantage in having the liquid strongly alkaline during the distillation ; but we render acid before titrating the residue.

The following are the working details :—

A standard solution of permanganate of potash (strength 0·4 milligramme of active oxygen per 1 cubic centim.) is prepared.

A standard *reducing-solution*, containing protosulphate of iron, and of such a strength that each cubic centim. exactly corresponds to the permanganate solution, is also prepared.

Solution of caustic potash, 5 per cent., is prepared.

Diluted sulphuric acid (1 volume of oil of vitriol with 9 volumes of water) is prepared.

These solutions having been got into order, the operator may begin.

A litre of the water to be examined is placed in a retort which is conveniently mounted, just as in the ordinary course of water-analysis by the ammonia process. Into the retort the operator drops with a graduated pipette 5 cubic centims. of the solution of potash (to render the water alkaline), and then 5 cubic centims. of very carefully measured standard solution of permanganate ; and then the contents of the retort are boiled by means of a large Bunsen burner, and the water is rapidly distilled off. After the distillation has advanced, it will very often happen that the permanganate will show signs of having been used up : if that happens, a second, and, if necessary, a third 5 cubic centims. of standard permanganate must be dropped into the retort, and the distillation continued until 800 or 900 cubic centims. have distilled over, and only some 200 or 100 cubic centims. of liquid remain behind in the retort. When the distillation is stopped, the observation that the contents of the retort still contain unacted-upon permanganate must hold good ; otherwise too little permanganate would have been employed in the operation.

The manner of finishing the operation is of a very obvious description. The operator acidifies the residue in the retort (*i. e.* he adds 10 cubic centims. of the diluted sulphuric acid) ; then he adds a measured quantity of iron-solution, taking care

to use a considerable excess; and finally he titrates back with the standard permanganate, and thus becomes provided with numerical data expressing how much oxygen has been used up by the litre of water.

We have just directed that 1 litre of water should be taken for an operation; and that quantity will be found convenient; but, as will strike the chemical reader who reflects on the conditions which limit the accuracy of analytical processes, there is nothing to forbid resort to a much larger scale if the highest degree of delicacy and accuracy were desired. In such a case *ten* litres *successively* introduced into the same retort would be advantageous.

We have worked on 4 litres *successively* introduced.

On applying the process, we have obtained the following results :—

	Oxygen consumed per litre of water.
Excellent distilled water	0·13 milligrm.
Thames water by a Water Company .	2·30 ,,

which show a striking difference between common drinking-water and water of the purest description.

Experiments on dilute solutions of cane-sugar have given results approximating very well to theory. Cane-sugar, indeed, we have subjected to a course of oxidations ranging over a very varied scale—from 5 milligrms. to upwards of 10 grms., at a single operation. The reaction is precise, viz. :—

(1) $C_{12} H_{22} O_{11} + O_{20} = 11 (H_2 O) + 4 (CO_2) + 4 (C_2 O_3)$;

(2) $C_{12} H_{22} O_{11} + O_{24} = 11 (H_2 O) + 12 (CO_2)$.
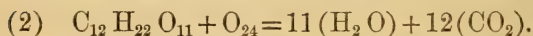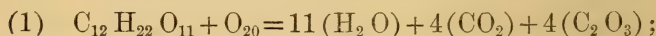
Equation (1) is valid if the action be restricted to the alkaline solution; and equation (2) is valid when the oxidation has been carried forward so as to be finished in the acid liquid. We postpone the publication of the experiments, and content ourselves with the general statement that we can by means of solution of permanganate effect perfect combustions of a number of organic compounds, the combustions taking place whilst the organic matter remains in aqueous solution.

Westminster, May 1878.

LXIV. *Notices respecting New Books.*

Popular Astronomy. By Simon Newcomb, *LL.D., Professor U.S. Naval Observatory.* London: Macmillan and Co., 1878.

THIS is by far the best-written work on Astronomy for the general reader we have seen for some time; its style is so lucid, and it is so full of sterling information, that a perusal of it cannot fail to impart a knowledge of the salient features of a branch of science which may well be considered the most ancient in the world. Dividing his subject into four parts, the author develops in each a leading astronomical idea, such as the earliest guesses at the construction of the external universe, the apparent and real motions of the heavenly bodies, and the forces by which order amongst them is maintained. Before treating of the groups of Planets, Satellites, Comets, and Meteors as illustrative of this order, he devotes the second part of his work to an exposition of the methods and instruments employed for obtaining a knowledge of the magnitudes, distances, and arrangements in space of the bodies by which we are surrounded. In the first chapter of this part, the author enters largely into the construction of the two classes of telescopes, Refracting and Reflecting, with an account of the principal great reflecting telescopes of modern times; in the second, his subject is the application of the telescope to celestial measurements; in the third, the determination of distances in the heavens; in the fourth, the motion of light; and in the fifth the construction and use of the spectroscope. The third and fourth parts of the work treat respectively of the Solar System and the Stellar Universe.

During the course of perusal we have noticed so many interesting aspects in which Professor Newcomb has presented Astronomy to his readers, that to particularize them would take up much more space than we can give; there are, however, a few that we cannot pass over. The peculiar construction of the Professor's mind fits him especially for discriminating between truth and error so far as a conception of the external world is concerned. He opens his work with an account of the apparent diurnal motion of the celestial sphere, and passes on to a description of the Ptolemaic system; and here we notice his acumen in clearly distinguishing between those views of Ptolemy which were correct and those in which he was in error; but perhaps the most interesting feature in connexion with Ptolemy is our author's showing that his system was a necessary step in the evolution of the true system of the universe subsequently developed by Copernicus, whose work was the greatest step ever taken in astronomy. From Copernicus he passes on to the work of Kepler and Newton, which culminated in the establishment of the doctrine of universal gravitation.

The portion of the universe most accurately known is "the Solar System;" and it is in this part of the work that we see the astronomical mind of the author strikingly developed: the subject-matter, while condensed, is at the same time remarkably full; not one im-

portant feature is lost sight of; every recent discovery is noticed, and its bearing on the general welfare of the system pointed out. In the chapter on the Sun the views of the most distinguished students of its physical constitution are given in full; and in connexion with the Solar System we would remark that the author clearly distinguishes between views that are destitute of support, those that are highly probable, and those which may be considered established by sufficient evidence. It is this feature which distinguishes every part of the work, and stamps it as the production of a mind of no ordinary calibre.

The portion of Professor Newcomb's work we have yet to notice is that in which he deals with the " Stellar Universe;" and it is here especially he brings out the paucity of our knowledge. In introducing it he says:—" The widest question which the study of the stars presents to us may be approached in this way: We have seen in our system of sun, planets, and satellites, a very orderly and beautiful structure, every body being kept in its own orbit through endless revolutions by a constant balancing of gravitating and centrifugal forces. Do the millions of suns and clusters scattered through space, and brought into view by the telescope, constitute a greater system of equally orderly structure? and if so, what is that structure? If we measure the importance of a question, not by its relations to our interest and our welfare, but by the intrinsic greatness of the subject to which it relates, then we must regard this question as one of the noblest with which the human mind has ever been occupied. In piercing the mystery of the Solar System, and showing that the earth on which we dwell was only one of the smaller of eight planets which move round the sun, we made a great step in the way of enlarging our ideas of the immensity of Creation and of the comparative insignificance of our sublunary interests. But when on extending our views we find our sun to be but one out of unnumbered millions, we see that our whole system is but an insignificant part of Creation, and that we have an immensely greater fabric to study. When we have bound all the stars, nebulæ, and clusters which our telescopes reveal into a single system, and shown in what manner each stands related to all the others, we shall have solved the problem of the material Universe, considered not in its details but in its widest scope."

    *   *   *   *   *   *   *   *

" Notwithstanding the amount of careful research which Herschel and his successors have devoted to it, we are still very far from having reached even an approximate solution of the problem of which we speak. In whatever direction we pursue it, we soon find ourselves brought face to face with the infinite in space and time. Especially is this the case when we seek to know, not simply what the Universe is to-day, but what causes are modifying it from age to age. All the knowledge that man has yet gathered is then found to amount to nothing but some faint glimmers of light shining here and there through the seemingly boundless darkness. The glimmer

is a little brighter for each successive generation; but many centuries must elapse before we can do much more than tell how the nearer stars are situated in space."

The contents of Part IV. relate to the stars as they are seen, views of the structure of the universe, and hypotheses explanatory of supposed changes in our system.

Treatise on Photography. By W. DE W. ABNEY, *F.R.S.* Longman and Co. 1878.

Instruction in Photography. By W. DE W. ABNEY, *F.R.S.* Piper and Carter, Gough Square, Fleet Street. 1876.

The very rapid and continuous progress which Photography has made, almost ever since the time of its discovery, a progress which of late years has been one of leaps and strides, would seem to have left its votaries but little time for its literature. It must be confessed that, in this country at least, the literature of photography is scanty; and this is not a little surprising when we consider either the artistic beauty of the results of photography, or its great importance in scientific research, or even the considerable commercial value of the interests involved in it.

We hailed with great satisfaction the announcement that Messrs. Longman's excellent series of Text-Books of Science was to receive an addition in the shape of a Treatise on Photography by one so competent for the task as Captain Abney, many of whose important original contributions to the Science have enriched the pages of this Journal. There is no lack of small manuals, of various degrees of excellence, which give instructions for producing photographs. In our experience the best of these is the small work by Captain Abney whose title we have given above. Based on a set of instructions originally drawn up by him for the Photographic School at Chatham, it has developed in two or three rapid editions into an excellent manual, which is, in the best sense of the word, "practical." It is far from being a mere compilation or transcript; on the contrary, the instructions given, when they are not original, bear the impress of being the result of a careful selection after actual experiment.

Captain Abney's later work fulfils a somewhat different function: it is designed for those who have already some knowledge of photography. The earlier chapters give a brief sketch of the history of the subject, which is followed by an account of the experiments on and properties of the spectrum which directly bear on photography, and an account of the changes which light produces on various substances submitted to its action.

Several chapters are devoted to a detailed account of the operations in various processes. Both here and in other parts of the work the author goes very fully into the *rationale* of the methods. Here, however, we may note some defects, or we should rather say excesses. In more than one instance the author goes beyond the facts of the case. Thus the equation for the action of potassium dichromate on organic matter, on page 31, is undoubtedly incorrect,

independently of the obvious misprints. We are not able to give a general equation for that action; but carbonic acid would certainly be formed; it may probably be affirmed that chromic acid would be reduced to chromic oxide, which would then combine with organic matter to form an insoluble compound. Where changes are possibly complicated, and certainly obscure, it would be better to explain them in somewhat general terms than attempt to tie them down in an equation.

The equation on page 141, for the action of gold chloride on sodium acetate in the toning-bath, can scarcely be admitted to be correct on the evidence that the solution becomes acid. This acid reaction may be accounted for in a variety of ways, one of the least probable being that trichloracetic acid is formed under these circumstances.

In the chapters on dry-plate and emulsion processes, and the like, the author has, it seems to us, rightly apprehended his task. He does not overload his book by an account of all possible modifications of these methods, but selects one or two approved typical ones, and describes and explains them with some fulness.

The principal methods of printing with inorganic salts other than silver are given, a branch of photography which, we think, has not received the attention it deserves. Here we would remark that in two or three cases, for instance in the account of Willis's aniline process, by giving the strengths of solutions used he would make his general descriptions not less readable, and at the same time convert it into a workable set of instructions.

In the account of the methods of printing in permanent pigment, and of the photomechanical processes, Captain Abney also preserves a right balance. There is a very useful chapter on lenses, and also one on the most approved forms of apparatus.

Chapter 31 is devoted to what may be called the æsthetics of photography,—to elucidating the principles which should guide the photographer in endeavouring to produce artistic pictures. A number of illustrations of such subjects by well-known photographers are given; and the minute analyses of the elements in these which go to produce the artistic effect are both ingenious and instructive. As something more than technical skill in painting is required to bring forth the highest results, so also in photography : although the process is so largely mechanical, technical proficiency alone will not produce satisfying results; a careful observance, however, of the principles laid down in this chapter will lead to the avoidance of the crude effects so often met with in photography, and so displeasing to the true lover of art.

The last chapters of the work are among the most valuable, as they are the most characteristic; they refer to the more purely scientific applications of photography, which the author has himself done so much to promote. Here are dealt with the subjects of actinometry, of photo-spectroscopy, of celestial and microscopic photography, and the miscellaneous applications (such as the automatic registration of meteorological and other physical phenomena). These

will be read with great interest; they abundantly vindicate the position of photography as a powerful engine of scientific research, one indeed on which we must depend for our future progress in many fields of inquiry.

Captain Abney has produced in his Treatise, in a small compass, a valuable addition to our scientific literature, and one with which no one who values the artistic and the scientific aspects of photography can well afford to dispense.

LXV. *Proceedings of Learned Societies.*

GEOLOGICAL SOCIETY.

[Continued from p. 395.]

March 6, 1878.—Henry Clifton Sorby, Esq., F.R.S., President, in the Chair.

THE following communications were read:—

1. "On the Geology of Gibraltar." By Prof. A. C. Ramsay, LL.D., F.R.S., and James Geikie, Esq., LL.D., F.R.S.

In this paper the authors, after giving some account of the physical features of Gibraltar, described in detail the various rock-masses of which the peninsula is composed. The chief rock is a pale grey bedded limestone, overlain by shales containing beds and bands of grit, mudstone, and limestone. Fossils are very rarely met with in the limestone, and have never as yet been found in the shales. The only recognizable fossil they obtained from the limestone was a *Rhynchonella*, which, Messrs. Etheridge and Davidson think, is most likely *Rh. concinna*. This would make the beds of Jurassic age. The limestone forms the great eastern escarpment, and dips west under the shales, which form the lower slopes upon which the town is built. The dips vary from 12° or 20° up to vertical. The connexion of these strata with the rocks of the adjoining districts in Spain and the opposite coast of Africa was traced; and it was shown that the Gibraltar Limestone reappears in Ape's Hill in Barbary, while the overlying shales and the sandstones of Queen-of-Spain's Chair form all the ground to the west of Ape's Hill up to Cape Spartel. The Jurassic strata of Gibraltar are overlain by various superficial accumulations, the oldest of which is a great mass of limestone-agglomerate, which is unfossiliferous, and shows as a rule no trace of stratification. It is made up of angular blocks of limestone of all shapes and sizes, and rests upon an uneven surface of limestone; it also covers wide areas underneath which only shales are present. It is excessively denuded, being worn into ravines and gullies, and presents generally a highly honeycombed surface. Terraces of marine erosion have also been excavated in it. It is not now accreting, and could not have been formed under present conditions of climate and surface. The authors gave at length their reasons for believing it to have been the result of a severe climate. The blocks were wedged out by the action of frost; and the heaps of angular *débris* thus formed were

saturated by water derived from melting snows, and so were caused to flow *en masse* down the mountain-slopes and over the gently inclined ground at their base.

The caves and fissures of Gibraltar were then described. It was shown that the true bone-breccias were confined to these. Many of these fossiliferous breccias are of later date than the great agglomerate, since they are met with in fissures and caves that intersect the limestone and limestone-agglomerate alike. When the mammalia tenanted Gibraltar, Africa and Europe were united, and the climate was genial.

All round the rock occur platforms, ledges, and plateaus, which are evidently the work of the sea. These erosion-terraces are covered in many places with calcareous sandstones containing recent species of Mediterranean shells. Such marine deposits occur up to a height of 700 feet. The movement of depression was interrupted by pauses of longer or shorter duration; and the climatic conditions were probably much the same as at present.

After the rock had been reelevated, the subaerial forces modified the surface of the marine sands that covered the limestone platforms, so that they came to form long sand-slopes. The land at this period was of greater extent than it is now, and some grounds exist for believing Europe to have been again united to Africa; for mammalian remains occur here and there in the deposits that overlie the limestone-platforms. These relics, however, it is just possible, may be derivative. The climate was probably still genial, like the present.

Overlying the marine and subaerial deposits just referred to occurs an upper and younger accumulation of massive unfossiliferous limestone agglomerate. This deposit the authors believe to owe its origin to severe climatic conditions. After the marine deposits that cloak so much of the eastern side of the rock had been weathered into subaerial sand-slopes, large blocks were detached from the cliffs and steep slopes; and these dropped down upon the sand and were soon drifted over. By-and-by the blocks fell in such quantities that the sand-slopes in many places were completely buried under a talus of limestone débris. This was subsequently consolidated by infiltration into a solid agglomerate, in the same way as the underlying sands were hardened into sandstone. These sandstones contain a few blocks of limestone only in their upper portions. In their horizontally bedded and lower-lying portions no limestone blocks occur.

This later agglomerate bears every stamp of great antiquity, and could not have been formed under present geographical and climatic conditions. The surface is honeycombed and worn, just like that of the solid limestone and the older limestone-agglomerate. Since its accumulation the climate has greatly changed, the present being characterized by the absence of frost.

In concluding the authors discussed at length the cause of the cold conditions that gave rise to the great limestone agglomerates, and argued that this cause could not have been *elevation of the land*. They also pointed out that a *submergence of the Sahara* would be

equally incompetent to bring about the desiderated climatic conditions, and that even a former much greater elevation of the land, combined with the appearance of a Sahara sea, would fail to supply us with the severe winter climate that was necessary to produce the great agglomerates. They thought that the most probable explanation of the phenomena described is that the cold conditions referred to were contemporaneous with that general refrigeration of climate which took place over so vast an area in our hemisphere during Pleistocene times. The limestone-agglomerates they look upon as the equivalents of those glacial deposits that occur so plentifully in our own and other countries; and the bone-breccias, which are intermediate in date between the lower and upper limestone agglomerates, are paralleled by the interglacial beds of the British Islands, Sweden, Switzerland, &c.

2. "Notes on the Geology of Japan." By J. G. H. Godfrey, Esq., F.G.S.

The author stated that Yesso, the most northern island of the Japanese group, had been geologically surveyed under the direction of Mr. Lyman, but that the geology of the other islands was chiefly known from Richthofen's general description. He finds that the classification of formations proposed by Lyman for Yesso holds good in all the other islands. Thus, going from newer to older deposits, he distinguishes :—

1. New alluvium, formed by existing rivers.

2. Old alluvium, formed by ancient rivers.

3. New volcanic rocks, consisting of basalt and rhyolite. Most of the Japanese volcanoes are extinct; but a few, such as Asamoyama, are in the solfatara stage; hot springs abound, and earthquakes are frequent.

4. Toshibetsu group, Middle or Lower Tertiary sandstones, clays, and conglomerates, containing lignite and petroleum.

5. Old volcanic series, rhyolitic rocks, often distinctly bedded, covering a vast area, and with numerous lodes and deposits containing gold, silver, copper, lead, and blende.

6. Horimui group, a coal- and lignite-bearing series of considerable extent, apparently best developed in the western part of Japan, and especially in the north of the island of Kiushiu, where the deposits are shown by fossil evidence to be of Cretaceous age.

7. Kamoikotan or metamorphic group, consisting of various schistose and gneissic rocks, distinctly stratified, and usually showing a dip of upwards of 60°. Owing to the absence of fossils the age of this group is still undecided; Richthofen regards it as Silurian or Devonian. Granite and diorite are frequently intruded into this series; and they contain some important mineral veins.

The author went into considerable details upon the useful minerals of Japan, noticing their mode of occurrence and the quantities in which they are produced. The most important of them are :— coal and lignite, copper, silver, gold, iron, petroleum, lead, and tin ; those of less consequence are sulphur (from the old craters), antimony, mercury, kaolin, and salt.

LXVI. *Intelligence and Miscellaneous Articles.*

CONTRIBUTION TO THE THEORY OF THE MOTION OF ELECTRICITY IN SUBMARINE AND UNDERGROUND TELEGRAPH-WIRES. BY G. KIRCHHOFF.

ASSUMING that the induction-effects produced by alterations of the current-intensity may be neglected against the influence of the charges of an underground telegraph-wire, Sir W. Thomson has referred the propagation of the electricity therein to the same laws as the conduction of heat. G. Kirchhoff develops this relation in connexion with the equations developed by Helmholtz respecting the components of the current-density ($u = -\lambda \dfrac{\partial \phi}{\partial x}$, &c., where λ denotes the conductivity) and of the electrostatic moment dependent on the dielectric polarizability* ($a = -k \dfrac{\partial \phi}{\partial x}$, &c.), if ϕ denotes the electrostatic potential, which is a function of x, y, z and consists of three parts, arising:—first, from the free electricity in and upon the conductor; secondly, from dielectric polarization; and, finally, from the double electric layer at the boundary surfaces of heterogeneous conductors. From the calculations, which cannot be given in abstract, it follows that

$$\Phi = e^{\beta z}\big(\mathrm{C} \cos{(nt + \alpha z)} + \mathrm{C}' \sin{(nt + \alpha z)}\big) + e^{-\beta z}\big(\mathrm{D} \cos{(nt - \alpha z)} + \mathrm{D}' \sin{(nt - \alpha z)}\big),$$

which equation represents two passages of waves in opposite directions along the z axis of the wire, in which the height of each wave as it moves forward diminishes correspondently to the value β. The period for ϕ according to the time is $\dfrac{2\pi}{n}$; β and α are given by the equations

$$\beta^2 - \alpha^2 = \frac{2\lambda}{\lambda_1 \rho_1{}^2 \log \dfrac{\rho_2}{\rho_1}},$$

$$\beta \alpha = \frac{n\mu}{\lambda_1 \rho_1{}^2 \log \dfrac{\rho_2}{\rho_1}},$$

where ρ_1 and ρ_2 are the internal and external radius of the gutta-perch sheath, λ and λ_1 the conductivities of it and the wire, $4\pi\mu = 1 + 4\pi k$ the constant of dielectricity of the gutta-percha.

Therefore the velocity of propagation of the waves increases with the conductivity of the gutta-percha, simultaneously with which their height diminishes as they travel onwards.

If the conductivity of the gutta-percha $\lambda = 0$, then becomes

$$\alpha = \beta = \sqrt{\frac{n\mu}{\lambda_1 \rho_1{}^2 \log \dfrac{\rho_2}{\rho_1}}}.$$

* Crelle's *Journal*, vol. lxxii.

If the wire is infinitely long, then (if, for $z = 0$, $\varphi = \cos nt$) is

$$\varphi = e^{-\beta z} \cos (nt - \alpha z).$$

Further, the following case is discussed :—that the wire possesses the length l, but has its termination connected with one of the coatings of a condenser, the other side of which is led away to earth. For the calculation in question, as well as the rest of the working, which cannot well be given in abstract, we must refer the reader to the original memoir.—*Beiblätter zu den Annalen der Physik und Chemie*, 1878, No. 1, vol. ii. pp. 221–223.

EXPERIMENT FOR ILLUSTRATING THE TERRESTRIAL ELECTRICAL CURRENTS. BY PROFESSOR WM. LEROY BROUN.

The following experiment enables a lecturer to exhibit to a large audience, in a very simple way, the action of the currents of electricity that pass around the earth. The experiment was suggested on reading an article by Professor J. W. Mallet, in the Philosophical Magazine for November 1877.

A rectangular frame was made of light poplar wood, of section three by two centimetres, whose sides were in length a fraction over a metre, and in breadth three fourths of a metre. About the perimetre of this rectangular frame were wrapped twenty coils of insulated copper wire; each extremity of the wire was made to terminate near the centre of one of the shorter sides, and passing through the wooden frame was fastened and cut off about three centimetres from the frame. This rectangular frame was then so suspended, in a horizontal position, by wires attached to the frame of an ordinary hydrostatic balance, that the longer sides were at right angles with the beam. By adjusting weights in the pans the index of the balance was brought to the zero-point. Two small orifices bored in a block of wood, a centimetre apart, served as mercury-cups, in which the extremities of the short terminal wires were immersed. Near the bottom and through the walls of these wooden cups were screwed small brass hooks, which served as connexions, to which the wires of the battery were attached. The balance was now so placed that the longer sides of the suspended rectangle were at *right angles with the magnetic meridian* or in the *magnetic* east-and-west line.

When the current from the battery was made to pass around the rectangle from east to west on the northern side, and from west to east on the southern side, by the theory of terrestrial magnetism the northern side of the rectangle would be attracted and the southern side repelled; and that this was so, the corresponding deflection of the balance rendered plainly visible. When the current was reversed the deflection was in the opposite direction. By breaking and closing the circuit at proper intervals to augment the oscillations, the large frame was readily made to oscillate through an arc of five degrees. When the sides of the rectangle were placed north-east and south-west the current produced no sensible

effect. A bichromate-of-potash battery of sixteen cells, with plates of zinc and carbon twenty-five by six centimetres, was used.

With a rectangle containing a larger number of coils of wire, attached to a very delicate balance, by using a *constantly acting* battery, the variation in the magnetism of the earth might thus be advantageously observed.—Silliman's *American Journal*, May 1878.

ON THE DIFFUSION OF CARBONIC ACID THROUGH WATER AND ALCOHOL. BY PROF. STEFAN.

When carbonic acid is parted from the atmospheric air by a liquid cylinder, it diffuses through the liquid into the external space. If the carbonic acid be kept under constant pressure, the rate of diffusion will be constant; the amount of carbonic acid issuing in the unit of time will be to the cross section of the cylinder directly, to its length inversely proportional.

The density of the carbonic acid diminishes from the inner to the outer boundary layer of the liquid which is saturated with the acid. The quantity of carbonic acid passing in the unit of time through the unit of the cross section is proportional to the fall in the density. The proportionality factor is the coefficient of diffusion, and can be determined from such observations.

A second method for determining it consists in the observation of the penetration of carbonic acid into a cylinder of liquid of great length. The amount of gas which has entered into the liquid from the commencement of the experiment up to a certain time is proportional to the square root of this time.

The velocities with which gases spread in liquids are of the same order as those with which salts diffuse in their solvents. The coefficient of diffusion of carbonic acid in water is nearly as great as that of potassium chloride. The coefficient for the diffusion of carbonic acid in alcohol is twice as great.

Oxygen and nitrogen diffuse in both liquids more quickly than carbonic acid; but the highest velocity of diffusion belongs to hydrogen gas. The peculiarities by which the gases are characterized in reference to their molecular motion in the free state, and which come out especially in their diffusion through porous solids, they still possess in the interior of liquids by which they have been absorbed.

That reciprocal action between gases and liquids, in consequence of which different gases are absorbed in different measure by one and the same liquid, has no influence upon the velocity of the diffusion-motion; the greater or less capacity of absorption of the gas determines in each given case the density of the diffusion-current only.—*Kaiserliche Akademie der Wissenschaften in Wien, mathematisch-naturwissenschaftliche Classe*, March 21, 1878.

INDEX TO VOL. V.

END OF THE FIFTH VOLUME.

Fig. 1.

Fig. 2.

Temperature Negative ← Y → Temperature Positive

Dotted Lines .... are Capacity Curves.
Continuous Lines _____ are Conductivity Curves.
Point A about 0·002 micro-farads capacity per cubic centimetre, temperature −12°·0 C.
Point E about 0·1185 micro-farads capacity per cubic centimetre, temperature +5° C.
Point F about 2240 megohms resistance per cubic centimetre, temperature −12°·4 C.
Point K about 0·34 megohms resistance per cubic centimetre, temperature +11°·02 C.

Ice as an Electrolyte.

W.E.Ayrton
John Perry

Mintern Bros. lith.

Fig. 1.

Fig. 3.
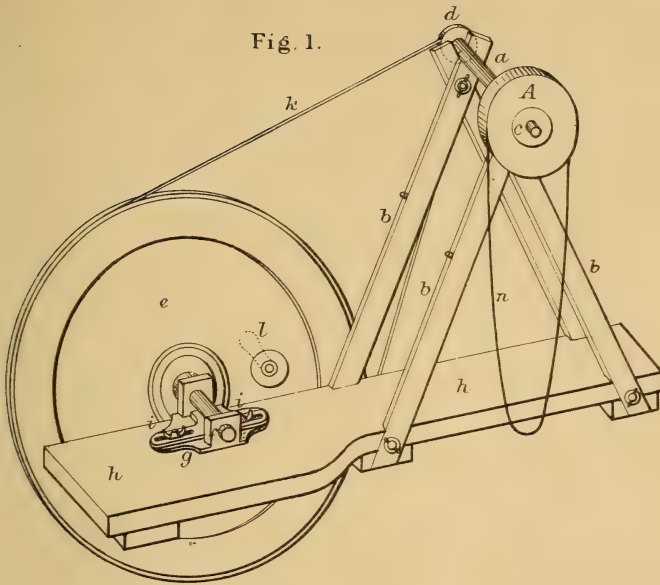
Fig. 2.
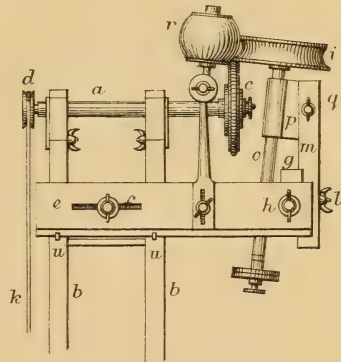
Fig. 4.
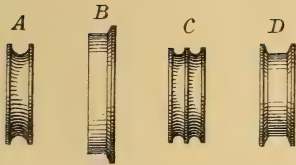
Fig. 5.
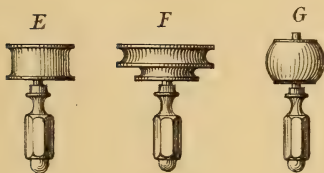
Fig. 1.
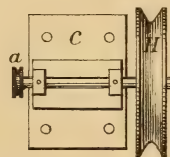
Fig. 2.

Fig. 5.

Fig. 3.

Fig. 4.

Scale 1 inch ══ 1 foot.

Fig. 1.
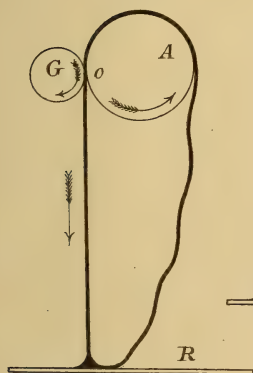
Fig. 2.
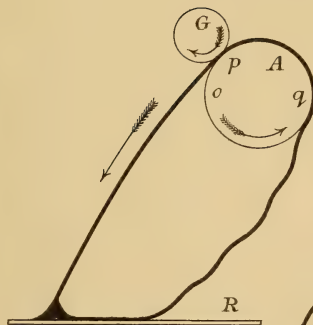
Fig. 3.
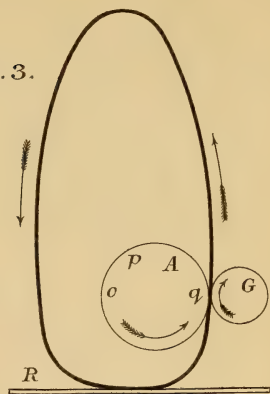
Fig. 4.
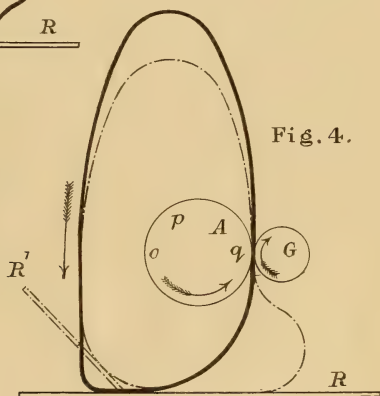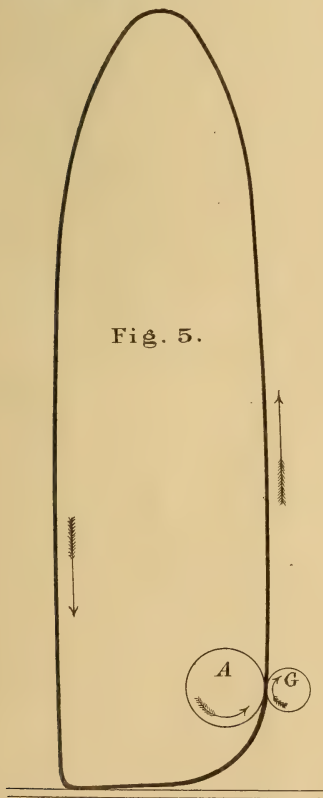
Fig. 5.

Fig. 6.

Fig. 1.

Fig. 2.

Fig. 3.

Fig. 4.

Fig. 1.

Fig. 2.

Half Size.

Fig. 3.

Fig. 4.

Fig. 6.

Fig. 7.

Fig. 5.

Fig

Fig. 3.

othermal.

M

p

v

V

New Sol

Fig. 4.

P

T+dT

N   T   M

O   V

g. 6.

Fig. 7.

P

K

L

J

N   M

O   V

V

10.

n

Water

Ice

P

Fig. 8.

T

O   V

Fig. 11.

P F S   W   D

Water   Water
and
Steam

Ice   Steam

J K   Ice
and
R   Steam

O   V

13.

M N   L

p

v   v
1   2   V

Fig. 14.

New Solution

Drops to fall on zinc plate

Overflow pipe

Carbon. Zinc Carbon.

Mercury

Waste tap

Waste bucket

Fig. 1.

Fig. 2.

F. 3.

Isothermal.

Fig. 4.

$T + dT$

Fig. 5.

+100

+ 4°

0°

Fig. 6.

Fig. 7.

Fig. 9.

Water

Water
and
Steam

Steam

Ice and Water

Ice

Ice
and
Steam

Fig. 10.

Steam

Water

Ice

Fig. 8.

Fig. 12.

Fig. 13.

Fig. 11.

Water

Water
and
Steam

Steam

Ice

Ice
and
Steam

F ig:1.

Fig:2.

Fig:3.

Fig:6.

Fig. 5.

Fig: 4.

Fig: 7.

Sir G. Airy's Correction of the Compass in Iron Ships, without use of a fixed mark.
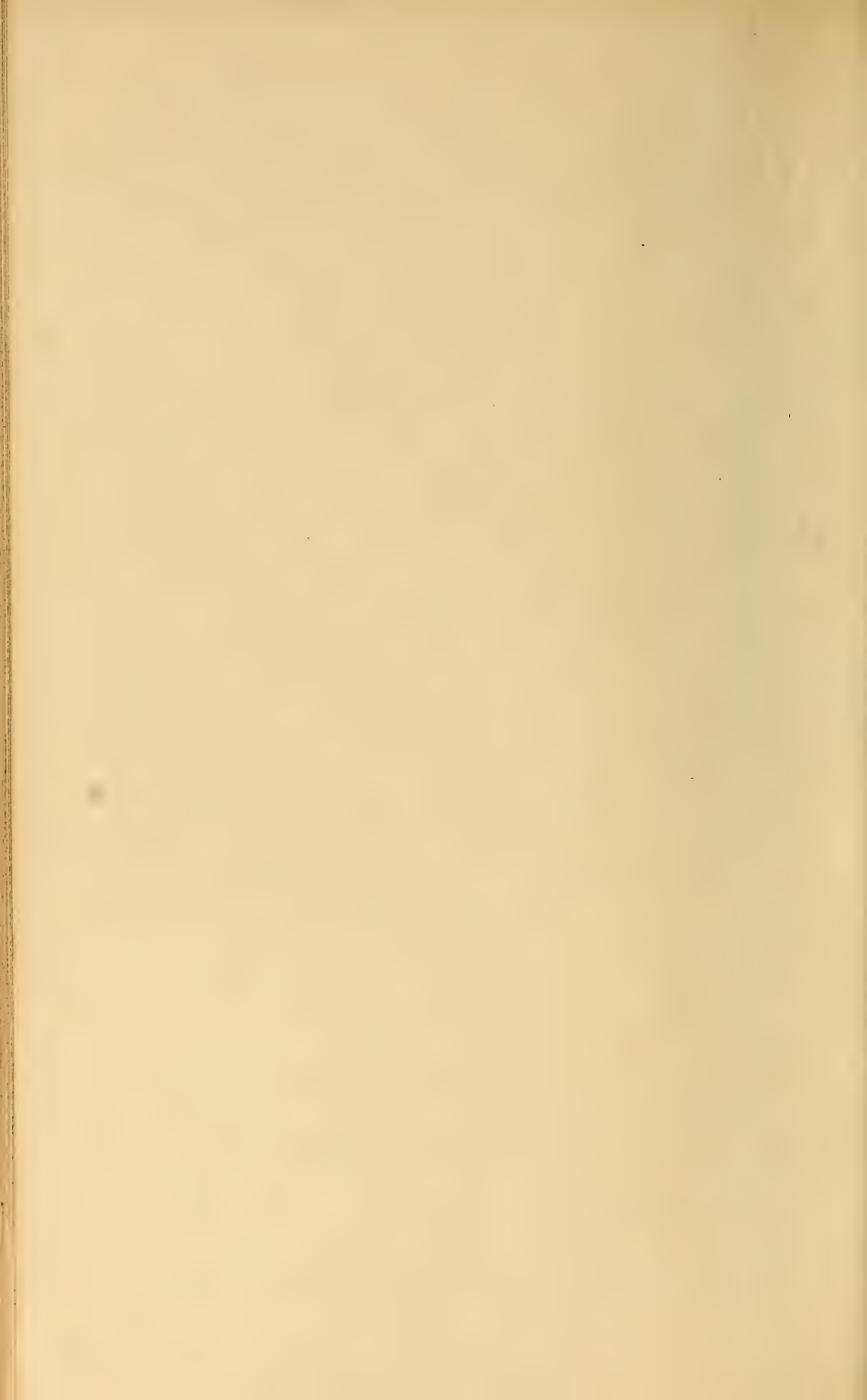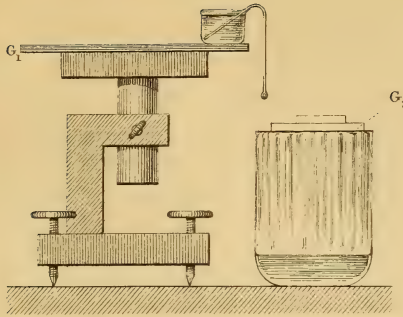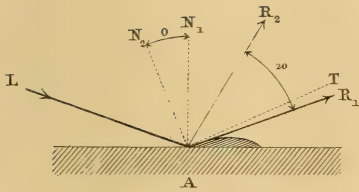
(Graduated Circle engraved on paper)

(Metallic T-Square.)

Fig. 1.

Fig. 2.
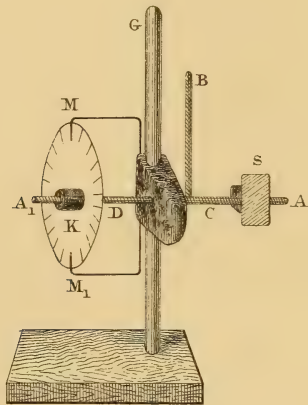
Fig. 3.
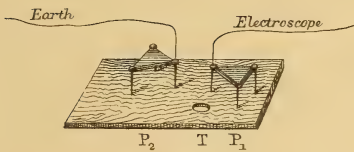
Fig. 4.
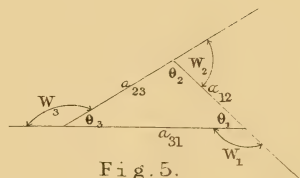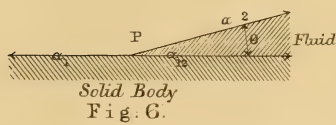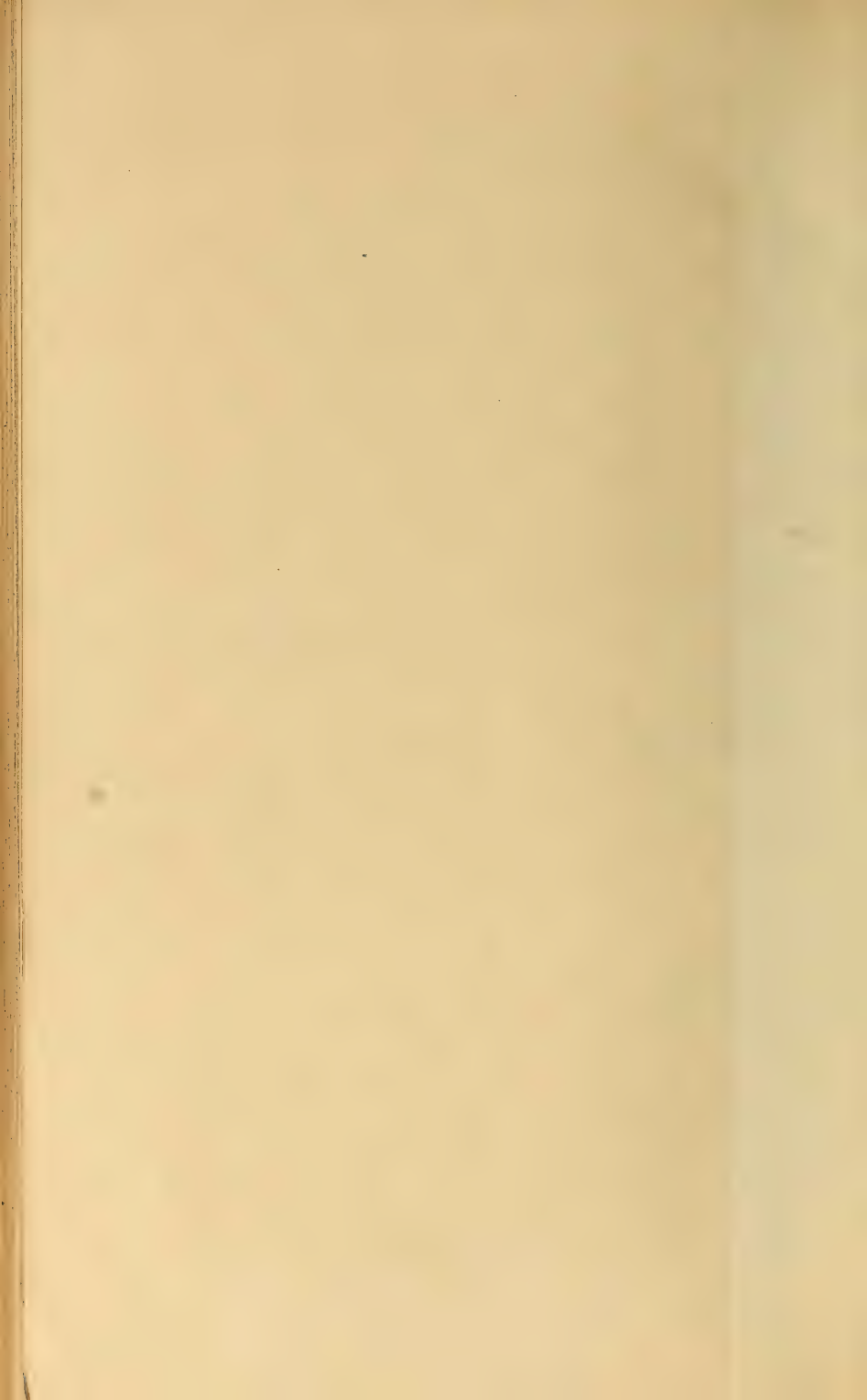
Fig. 5.

Fig. 6.

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April 1878.

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